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EDITORIAL NOTES.

AT the Annual General meeting of the Geological Society of London on February 18 last, the officers for the ensuing year were elected as follows: President, Mr. R. D. Oldham (second year of office); Vice-Presidents, Professor E. J. Garwood, Mr. G. W. Lamplugh, Col. H. G. Lyons, and Dr. G. T. Prior; Secretaries, Dr. H. H. Thomas and Mr. W. Campbell Smith; Foreign Secretary, Sir A. Geikie; Treasurer, Mr. R. S. Herries. The new members of the Council are Mr. T. C. Cantrill, Mr. J. A. Douglas, Dr. J. S. Flett, Sir A. H. McMahon, Professor S. H. Reynolds, and Sir A. Strahan.

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AN important representative Committee has been formed to raise a suitable testimonial to Professor Percy Fry Kendall, M.Sc., of the University of Leeds. The Chair of Geology has been occupied by him for thirty years to the great benefit of all those students who have been stimulated by his brilliant and enlightened teaching. He has also done important work in Glacial Geology, and rendered valuable assistance on both the scientific and economic sides of the geology of the Coalfields, in addition to other interesting and useful investigations and the popularizing of geological science by lectures and addresses in Yorkshire and the north of England. Subscriptions should be made payable by cheque or otherwise to the Hon. Treasurer, J. E. Bedford, Esq., F.G.S., Arncliffe, Shire Oak Road, Headingley, Leeds.

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BY the death of Mr. Charles Edward Fagan, C.B.E., I.S.O., Secretary of the British Museum (Natural History) on January 30, aged 65 years, science has been deprived of the active services of an executive officer who for years past had exerted himself very warmly on behalf of the National Museum. He rendered valuable aid in sending off expeditions to various regions and in obtaining the necessary funds for the publication of the scientific results obtained and ensuring the acquisition and preservation of the new and rare specimens met with. Mr. Fagan entered the Museum in 1873, was made assistant secretary in 1889, and secretary in 1919. He would have retired this spring.

Founders of Seismology.—I. John Michell.

By Charles Davison, Sc.D.

To the earthquake that ruined Lisbon on November 1, 1755, we may trace the great interest in the study of earthquakes which marks the middle of the eighteenth century, though valuable accounts of previous earthquakes had already appeared in the pages of the *Philosophical Transactions*. During the following year the Royal Society paid the same close attention to this earthquake that it did more than a century later to those of the eruption of Krakatoa. In 1757 an anonymous editor published *The History and Philosophy of Earthquakes*, in which he gave abstracts of ten memoirs by the "best writers on the subject"—Hooke, Woodward, Buffon, and Stukeley among them—followed by a summary of the observations on the Lisbon earthquake that had been communicated to the Royal Society. These are the principal sources on which Michell depended when he wrote his memoir on the cause and phenomena of earthquakes.[1]

The date and place of Michell's birth are unknown, but the year must have been 1724 or 1725, and the place may have been Nottingham. Coming, at any rate, from that city, he entered Queens' College, Cambridge, in 1742, and graduated as fourth wrangler in the second mathematical tripos (1748–9). This success, which but for his wide interests would probably have been greater, was followed by his election to a fellowship at Queens'. His wonderful versatility, even at so early a stage, is shown by his ability to give college lectures on Greek and Hebrew as well as on mathematics. At this time, too, he must have been making those original observations on the structure of the earth's crust, which are described in his memoir on earthquakes. Two years later he was appointed Woodwardian Professor of Geology, being the third holder of that office and the most capable until Sedgwick began his long reign in 1818. In addition to the college tutorship and lectures—there is no evidence that the duties of the professorship were a serious burden—Michell held the rectory of St. Botolph's, Cambridge, from 1760 to 1763. The latter year, however, brought a change, the busy official life at Cambridge giving place to the studious quiet of a country rectory, first at Compton, near Winchester, then at Havant, and lastly, in 1767, at Thornhill, a Yorkshire village not far from Dewsbury. The fellowship at Queens' and the professorship of geology were both vacated in 1764, the year of his first marriage, and no doubt as a

[1] "Conjectures concerning the Cause and Observations upon the Phenomena of Earthquakes; particularly of that great Earthquake of the First of November, 1755, which proved so fatal to the City of Lisbon, and whose Effects were felt as far as Africa, and more or less throughout all Europe." (Read February 28, March 6, 13, 20, 27, 1760.) *Phil. Trans.*, vol. li, 1761, pp. 566–634.

result of that step. In a smaller man such a life might have ended in stagnation, but, with his musical evenings at home, his friendship with many leading men of science, and his frequent visits to London, Michell maintained his freshness, and science rather gained than lost by the change. Happy in his social life, unfailing in the discharge of his parochial duties, and evidently possessing means sufficient for his wants, Michell carried on his scientific work without pause and without hurry, touching nothing that he did not adorn, and careless, apparently, to whom the credit might fall so long as the work was done. He died at Thornhill on April 21, 1793, in his 69th year.[1]

Except for his early *Treatise of Artificial Magnets* and his memoir on earthquakes, Michell's work was mainly astronomical. At the age of 25 he discovered the method of making magnets by double touch and the variation of magnetic action according to the inverse square of the distance. Less than ten years later he developed those clear views on stratification that are fully appreciated by modern geologists. Equally original and no less important was his memoir on stellar parallax (1767). In this he insisted on the extreme minuteness of the parallax of even the brightest stars, and inferred that the nearest fixed star is probably no farther from us than 220,000 times the sun's distance—a quite close, if accidental, approximation to the distance of α Centauri. The apparent proper motion of the stars he correctly attributed in part to that of the sun; he estimated the extreme probability that the stars which form the Pleiades constitute a connected system, and he foresaw the discovery, made shortly afterwards by Herschel, of the revolutions of the double stars. At Thornhill he experimented on the best alloys for the mirrors of reflecting telescopes and on the method of giving the mirrors their proper form. The 10 ft. reflector which he made was afterwards bought and used by William Herschel. Michell also invented the torsion-balance, and devised a method for determining the density of the earth. The apparatus was completed a short time before his death, but he did not live to make any experiment with it. It passed ultimately into the capable hands of Henry Cavendish, and led to his well-known determination of the density of the earth. Michell, like Darwin, was evidently one of those men who "seemed by gentle persuasion to have penetrated that reserve of nature which baffles smaller men". Is it too much to claim for him a place, not, indeed, beside Newton and Darwin, but in a rank not so very far below them?

During his own life Michell's extraordinary power was known and valued. Among his friends he could number such men as Cavendish, Black, Priestley, and William Herschel. It is all the stranger, then, that his memoir on the cause and phenomena of

[1] The above biographical details are derived chiefly from articles in the *Dictionary of National Biography*; the *English Mechanic*, vol. xiii, 1871, pp. 309–10; *Knowledge*, vol. xv, 1892, pp. 188–91, 206–8; and Sir A. Geikie's *Memoir of John Michell* (Camb. Univ. Press), 1918.

earthquakes should have remained, or appeared to remain, practically unknown for more than half a century. It was referred to in an article in the *Edinburgh Review* for February, 1818, and later in that year was reprinted, with notes by John Farey, sen., in the *Philosophical Magazine*.[1] Many years after his death, full, if belated, justice has been done to Michell's stratigraphical conceptions; but his work, so far as it deals with earthquakes, is less well known, owing partly perhaps to the inaccurate account which Mallet gave of it in his early memoirs.[2]

Leaving out of account the important section on the nature and trend of strata—a section which has been adequately dealt with by Sir A. Geikie and others—Michell's paper may be divided into three parts. In the first he describes the phenomena of earthquakes, in the second he explains in detail his theory of their cause, and in the third he gives methods for determining the position of the seismic focus.

The Phenomena of Earthquakes.—Like his predecessors, Michell begins with a summary of the phenomena which his theory must explain; but, unlike them, he distinguishes between certain phenomena which are essential to, and others which have no connexion with, the origin of earthquakes. The latter are what we now call secondary or mechanical effects of the shock, such as the sudden stopping and gushing out of fountains occasioned by the opening or contraction of fissures, the dizziness and sickness induced by the wave-like motion, and the disturbances sometimes occasioned in the direction of the magnetic needle. These are not "observed to be constant attendants on earthquakes, nor do they seem materially to affect the solution given one way or other". The phenomena which Michell regarded as essential, five in number, are the following:—

(i) The same places are subject to returns of earthquakes, not only at small intervals for some time after any considerable one has happened, but also at greater intervals of some ages. Michell thus distinguishes between the occurrence of after-shocks and the returns of great earthquakes, as he shows by his reference to the 451 shocks felt at Lima from October 28, 1746, to February 24, 1747, and to the sixteen very violent earthquakes at the same place from 1582 to 1746.

(ii) Those places that are in the neighbourhood of burning mountains are always subject to frequent earthquakes, and the eruptions of those mountains, when violent, are generally attended by them. Michell refers especially to Peru and Chile, no other known countries, he supposes, being so subject to earthquakes and none so full of volcanoes.

[1] Vol. lii, 1818, pp. 186–95, 254–70, 323–40.

[2] *Trans. Irish Acad.* (read 1846), vol. xxi, 1848, pp. 58–60, 65–7, 84–5; also *Rep. Brit. Assoc.*, 1850, pp. 17–19.

(iii) The motion of the earth in earthquakes is partly tremulous and partly propagated by waves, which succeed one another sometimes at larger, sometimes at smaller, distances; and this latter motion is generally propagated much farther than the other. The former part of this statement, Michell considers, "wants no confirmation." Indeed, his explanation of the manner in which the vibrations are propagated through the solid crust is quite as clear as that given by Mallet nearly a century later. In illustration of the latter, he quotes accounts of what are now called "visible waves", such as that of an observer of the Jamaica earthquake of 1688, who "saw the ground rise like the sea in a wave as the earthquake passed along, and" who "could distinguish the effects of it to some miles distance by the motion of the tops of the trees on the hills". In the Lisbon earthquake "this wave-like motion was propagated to far greater distances than the other tremulous one, being perceived by the motion of waters and the hanging branches in churches through all Germany . . . in Denmark, Sweden, Norway, and all over the British Isles".

(iv) It is observed in places which are subject to frequent earthquakes that they generally come to one and the same place from the same point of the compass. Also the velocity with which they proceed (as far as one can collect it from the accounts of them) is the same; but the velocity of the earthquakes of different countries is very different. Michell's illustration of the latter statement, though not quite accurate, is interesting. The velocity with which the Lisbon earthquake and its after-shocks were propagated "was the same, being at least equal to that of sound; for all followed immediately after the noise that preceded them, or rather the noise and the earthquake came together"; that of the New England earthquakes was less, as is shown by the longer interval between the preceding noise and the shock.[1]

(v) The great Lisbon earthquake was succeeded by several local ones (in Switzerland and elsewhere), the extent of which was much less.

The Cause of the Earthquakes.—Long before Michell's time, it was "the general opinion of philosophers that earthquakes owe their origin to some sudden explosion in the internal parts of the earth".[2] Michell traces earthquakes to the primary cause to which they also appeal, namely, subterraneous fires. He differs from his predecessors chiefly in his endeavour to support this theory by facts and to trace out the effects of such explosions. "These fires," he says, "if a

[1] From records of the time of the great earthquake at Lisbon and more distant places, Michell estimates the velocity at more than 20 miles a minute. This is the earliest estimate of the kind known to me.

[2] The following sentence is worth quoting from *The History and Philosophy of Earthquakes* (p. 26), as it refers to probably the earliest instance of a seismic experiment: "Honoratus Faber illustrates this doctrine by a variety of artificial earthquakes, as he calls them, confining gunpowder (a mixture of nitre, sulphur, and charcoal) in pits, and setting fire to it by a train."

large quantity of water should be let out upon them suddenly, may produce a vapour whose quantity and elastic force may be fully sufficient for the purpose." Indeed, he remarks, "it is not easy to find any other cause capable of producing such sudden and violent effects."

The fires to which he refers are of the same kind as the fires of volcanoes, but, like Humboldt after him, Michell regards the volcanic fires as safety-valves, "for volcanoes, giving passage to the vapours that are thus formed, should rather prevent" earthquakes. If it should be asked why we should suppose that subterraneous fires exist in the neighbourhood of volcanoes, Michell points to the frequent instances of new volcanoes breaking out near old ones, to the existence of many volcanoes close together in the same tract of country, and to the rarity of isolated volcanoes.

Michell regards it as very probable that the fires of volcanic regions originate in a stratum, like coal or shale, in which pyrites is lodged in such quantity that it will inflame of itself, but that the quantity varies in different parts of the stratum so that the fires are not continuous. The same strata, he remarks, are generally very extensive, and they commonly lie more inclining from the mountainous countries than the countries themselves. "These circumstances make it very probable that those strata of combustible materials which break out in volcanoes at the tops of the hills are to be found at a considerable depth under ground in the level and low countries near them. If this should be the case, and if the same strata should be on fire in any places under such conditions, as well as on the tops of hills, all vapours, of whatsoever kind, raised from these fires must be pent up, unless so far as they can open for themselves a passage between the strata; whereas the vapours raised from volcanoes find a vent, and are discharged in blasts from the mouth of them. Now, if when they find such a vent they are yet capable of shaking the country to the distance of ten or twenty miles round, what may we expect from them when they are confined?" Thus, "the most extensive earthquakes should take their rise from the level and low countries, but more especially from the sea, which is nothing else than waters covering such countries."

The manner in which the water contained in fissures reaches the subterraneous fires below is explained at some length, but in parts with less than Michell's usual clearness. He supposes the roof over a subterraneous fire to fall in. "If this should be the case, the earth, stones, etc., of which it was composed, would immediately sink in the melted matter of the fire below; hence all the water contained in the fissures and cavities of the part falling in would come in contact with the fire and be almost instantly raised in vapour." The fall of the roof, Michell admits, could not happen accidentally. He connects it with the expansion of the matter of which the subterraneous fire is composed, by which the roof is gradually raised and bent until collapse takes place.

He then proceeds to show how his theory will account for the various phenomena described above. The return of earthquakes to the same place is to be expected, for, as in volcanoes, the subterraneous fires may be supposed to subsist for many ages. Moreover, as volcanoes frequently rage for a time and then are quiet for a number of years, so we see earthquakes frequently repeated for some small time and then ceasing for a long period, except perhaps for the occurrence now and then of some slight shock. The great frequency of earthquakes in the neighbourhood of burning mountains is a strong argument of their proceeding from a cause of the same kind.

The two forms of motion which Michell so clearly discerned in earthquakes are consequences, in his view, of the varying amount of the vapour generated. A small quantity of vapour almost instantly generated would produce the vibratory motion; a very large quantity would result in a wave-like motion.

The first effect of the vapour, he says, would be to form a cavity between the melted matter and superincumbent earth by the compression of the materials immediately over the cavity, and "this compression must be propagated on account of the elasticity of the earth, in the same manner as a pulse is propagated through the air; and again the materials immediately over the cavity, restoring themselves beyond their natural bounds, a dilatation will succeed to the compression; and these two following each other alternately for some time a vibratory motion will be produced at the surface of the earth. If these alternate dilations and compressions should succeed one another at very short intervals, they would excite a like motion in the air, and thereby occasion a considerable noise. The noise," he adds, "that is usually observed to precede or accompany earthquakes is probably owing partly to this cause, and partly to the grating of the parts of the earth together, occasioned by that wave-like motion before mentioned."

Michell illustrates the propagation of the wave-like motion by describing an experiment which has led to some misconception of his views. "Suppose a large cloth or carpet (spread upon a floor) to be raised at one edge and then suddenly brought down again to the floor, the air under it, being by this means propelled, will pass along until it escapes at the opposite edge, raising the cloth in a wave all the way as it goes. In like manner, a large quantity of vapour may be conceived to raise the earth in a wave, as it passes along between the strata, which it may easily separate in an horizontal direction, there being . . . little or no cohesion between one stratum and another. The part of the earth that is first raised, being bent from its natural form, will endeavour to restore itself by its elasticity, and the parts next to it, beginning to have their weight supported by the vapour which will insinuate itself under them, will be raised in their turn, till it either finds some vent or is again condensed by the cold into water, and by that means prevented from proceeding any further."

Equally interesting, and less open to objection, is Michell's explanation of the propagation of the wave-like motion to distances far greater than the vibratory motion. This motion, he states, "will be propagated through the solid parts of the earth, and, therefore, it will much sooner become too weak to be perceived than the wave-like motion, for this latter, being occasioned by the vapour insinuating itself between the strata, may be propagated to very great distances; and even after it has ceased to be perceived by the senses it may still discover itself by the appearances before mentioned." That is to say, in modern terms, the wave-like motion is propagated in two and the vibratory motion in three dimensions.

All earthquakes derived from the same subterraneous fire must come to any place in the same direction. Those only which are derived from different fires will come from different points of the compass. Moreover, since, according to Michell, the velocity with which the vapour insinuates itself between the strata depends upon its depth below the surface, earthquakes must travel with the same velocity if they proceed from fires at different depths. The occurrence of local earthquakes in other places Michell accounts for by supposing that the vapour coming from one fire may disturb the roof over some adjacent fires, and so occasion earthquakes by the falling in of some part of it.

Michell also suggests explanations of two other phenomena besides those described above. These are the great sea-waves and "a sort of periodical return" of earthquakes in the same region. The first movement of the sea at Lisbon was, as usual, a retreat of the water followed by the inrush of the great waves. During the elevation of the roof over the subterraneous fire the waters of the ocean lying over it must retreat, but so slowly and so gently as to occasion no great disturbance. But as soon as one part of the roof falls in "the cold water contained in the fissure of it, mixing with the steam, will immediately produce a vacuum, in the same manner as the water injected into the cylinder of a steam-engine, and the earth subsiding, and leaving a hollow place above, the waters will flow every way towards it and cause a retreat of the sea on all the shores round about; then, presently, the waters being again converted by the contact of the fire into vapour . . . the earth will be raised, and the waters over it will be made to flow every way and produce a great wave immediately succeeding the previous retreat."

Lastly, the great quantity of water let out upon the subterraneous fire "must extinguish a great portion of the burning matter, in consequence of which it will be contracted within much narrower bounds", and this will continue "till the roof is well settled and the surface of the melted matter sufficiently cooled, after which it may require a long time for the fire to heat it again so much as will be necessary to make it produce the former effects".[1]

[1] At the end of the section on stratification Michell gives the following paragraph: "Besides the rising of the strata in a ridge, there is another very

The Position of the Epicentre.—In the last section of his memoir, Michell shows that "if we would inquire into the place of the origin of any particular earthquake, we have the following grounds to go upon:—

(i) "The different directions in which it arrives at several distant places; if lines be drawn in these directions the place of their common intersection must be nearly the place sought; but this is liable to great difficulties, for there must necessarily be great uncertainty in observations, which cannot, at best, be made with any great precision, and which are generally made by minds too little at ease to be nice observers of what passes; moreover, the directions themselves may be somewhat varied by the inequalities in the weight of the superincumbent matter, under which the vapour passes, as well as by other causes.

(ii) "We may form some judgment concerning the place of the origin of a particular earthquake from the time of its arrival at different places, but this also is liable to great difficulties. In both these methods, however, we may come to a much greater degree of exactness by taking a medium amongst a variety of accounts as they are related by different observers. But,

(iii) "We may come to the greatest degree of exactness in those cases where earthquakes have their source from under the ocean; for in these instances the proportional distance of places from that source may be very nearly ascertained by the interval between the earthquake and the succeeding wave, and this is the more to be depended on as people are much less likely to be mistaken in determining the time between two events which follow one another at a small interval than in observing the precise time of the happening of some single event."

The first method is that which Mallet applied with such skill in his study of the Neapolitan earthquake of 1857, though he depended on more permanent evidences of the direction than the impressions of observers. The third method is, of course, vitiated by the assumption that the velocity of sea-waves is independent of the depth of the ocean, but the greater accuracy attainable by measuring intervals rather than absolute times lies at the root of the method depending on the duration of the preliminary tremor which is now so widely used.

remarkable appearance in the structure of the earth, though a very common one; and this is what is usually called by miners the trapping down of the strata; that is, the whole set of strata on one side such a cleft are sunk down below the level of the corresponding strata on the other side. If, in some cases, this difference in the level of the strata on the different sides of the cleft should be very considerable, it may have a great effect in producing some of the singularities of particular earthquakes." One cannot help regretting the brevity of Michell's statement on the relations between earthquakes and faults. I can only suggest that, according to his view, the vapour, in travelling outwards by parting adjacent strata, would be suddenly arrested at the fault.

In locating the origin of the Lisbon earthquake Michell relies on other evidence rather than on the methods which he describes so clearly. The great sea-wave which followed the earthquake indicates that the origin was submarine, and it could not be far from Lisbon, as the wave arrived there so soon after the earthquake and was of such great height. It must, he thinks, lie somewhere between the latitudes of Lisbon and Oporto, though probably somewhat nearer to the former (about a degree of a great circle, he afterwards assumes, from Lisbon and a degree and a half from Oporto), and at a distance of 10 or 15 leagues from the coast. Such a position agrees well with the few observations which he possessed on the direction and time of the earthquake. But, curiously enough, he notices that "the times which the wave took up in travelling are not in the same proportion with the distances of the respective places from the supposed source of the motion". He does not, however, regard this as an objection to the point assumed, but thinks—and here he foresaw a later discovery—that "the true reason of this disproportion seems to be the difference in the depth of the water; for in every instance in the above table the time will be found to be proportionately shorter or longer as the water through which the wave passed was deeper or shallower".

The methods which Michell suggests for inquiring "into the depth at which the cause lies that occasions any particular earthquake" depend on his theory of the origin of earthquakes, and need not be here described. For the Lisbon earthquake the observations required in order to apply the methods did not exist. But "if", he says, "I might be allowed to form a random guess about it I should suppose (upon a comparison of all circumstances) that it could not be much less than a mile or mile and a half, and I think it is probable it did not exceed three miles."

Conclusion.—With our fuller knowledge it would be an easy matter to criticize Michell's theory of the cause of earthquakes. I do not propose to enter now on such a task, my object being rather to give a brief summary of his contributions, which seem to me to possess a permanent value.

In the first place must be mentioned his distinction between the phenomena which are essential and those which are not essential to his theory. In separating the vibratory motion from the wave-like motion, Michell was in advance of his time. He was one of the first, if not the very first, to assign the vibratory motion in earthquakes to the propagation of elastic waves in the earth's crust.

In the second place he rendered an important service in his attempt to give a consistent theory of the origin of earthquakes, even if the theory, as we can now see, rests on an erroneous conception of volcanic action, and if it cannot explain all the phenomena that have since been discovered. His suggestions as to the origin of earthquake sounds and of seismic sea-waves would seem, in part, at any rate, to be correct.

Lastly Michell devised two useful methods for determining the position of the epicentre. Both were afterwards overlooked and both have been reinvented and applied with varying success to the investigation of earthquakes. Michell's "random guess" as to the depth of the focus, like Newton's guess with regard to the density of the earth, is one of those intuitions that occur only to the ablest minds.

Michell evidently failed to impress his readers, and "he who succeeds in doing so", says Darwin, "deserves in my opinion all the credit." But what if the failure be the fault of his readers—not of his contemporaries, for it was chiefly to this memoir that Michell owed his election to the Royal Society, but of their successors? If Michell's conceptions of the form and trend of strata could sink into temporary oblivion, it is not surprising that his almost prophetic insight into earthquake phenomena should share their fate. But if his influence on future workers were less than his memoir deserves he does not, I think, forfeit his claim to be regarded as one of the Founders of Seismology.

ORIGINAL ARTICLES.

On Cycles of Sedimentation in the Eocene Strata of the Anglo-Franco-Belgian Basin.[1]

By L. Dudley Stamp, M.Sc., A.K.C., F.G.S., King's College, London.

I. INTRODUCTION.

THE term "Cycle of Sedimentation" or "Cycle of Deposition" is not nearly so familiar in England as it is on the Continent. Whilst the teaching of the general principles forms part of most courses in geology, the application of these principles to actual successions in this country has been somewhat neglected. Attention has recently been drawn to the seasonal or rhythmic changes seen in some sediments, and to avoid confusion it is desirable to outline what is meant by the expression "Cycle of Sedimentation".

If one considers a sea transgressing over a steadily sinking land, the first stage in its progress will be marked by coarse, conglomeratic deposits of coastal (not necessarily beach) origin. This basal "conglomerate" is the "gravier d'immersion" of French authors. It passes laterally in a seaward direction into sands and finer deposits. In a later stage the coastal deposits will overstep the former, and deeper-water sediments—sands and clays—be laid down in direct succession to the coastal deposits of earlier date. This process continues during the transgression, and so in deposits of a simple overlap of this nature one has, at any point, a vertical transition from pebble-beds through coarse sands to finer sands and clays. That the basal conglomerates are not of the same age throughout is proved by a study of the faunas. One finds a palæontological zone to pass laterally from clays into sands and coarse sands and probably into the coastal pebble-beds. Such zones are represented in the diagram (Fig. 1) by horizontal layers marked Z 1, Z 2, Z 3, etc.

When the transgressive movement of the sea ceases there is generally a tendency for the basin to become "silted up". This is especially the case when quantities of sediment are being brought into the basin from adjacent continental areas. In the neighbourhood of a delta one finds that the marine deposits pass gradually upwards into estuarine beds with a brackish-water fauna. These are succeeded in turn by lagoon (brackish or fresh water), lacustrine, fluviatile, or even æolian deposits. For the sake of brevity, all sediments of this nature, that is, other than marine, are referred to as *continental*.

In other parts of a basin of deposition where little sediment is being received from continental sources, the shallowing or, it may be,

[1] Paper read before Section C (Geology) of the British Association for the Advancement of Science, Cardiff Meeting, August, 1920.

retreat (regression) of the sea is marked by a recurrence of coarser sediments—silts and sands—culminating possibly in conglomerate (" gravier d'émersion ").

The succeeding marine invasion may remove a portion of the continental or coastal deposits, the coarse or hard materials being rearranged to form a new " gravier d'immersion " or basal conglomerate. This frequently includes rolled fragments of fossils, etc., derived from the underlying beds. Where the latter are fine-grained or unconsolidated, the " gravier d'immersion " may be feebly developed and inconspicuous, but the plane of erosion between the new marine deposits and the underlying beds, whether of continental or marine origin, is almost invariably well-marked. In other words there is a " ravinement " at the base of each marine formation.[1]

The period between two successive " ravinements " constitutes a " cycle of sedimentation ". In other words, a " cycle of sedimentation " comprises the deposits of a complete oscillation of the basin, each oscillation including a positive phase of marine invasion and a negative phase of regression.

The deposits of the second marine invasion form a series closely comparable with the first. The invasion may be more or less extensive than the earlier, and the seaward range of the continental deposits more restricted or less so.

Fig. 1 is a diagrammatic representation of the deposits of three complete cycles of sedimentation. The section is drawn from the landward edge of a basin towards the centre. The relative extent of marine and continental deposits is seen at a glance. Taking the upper or lower limit of the zones as representing approximately time planes, it is apparent that each basal conglomerate is not of the same age over its whole extent. That is, the cycle does not commence at exactly the same time at each point. Yet, as many authors have recognized, the only possible grouping of such deposits is by cycles. The changes at any particular locality during a series of cycles may be represented graphically. Taking a straight line to represent sea-level and regarding, for the sake of clearness, all continental deposits as having been formed above sea-level, the oscillations of the land surface at any one point may be represented by a curve. This graphical representation is important in that it emphasizes the time factor. Two examples, for the points *A* and *B*,

[1] A " ravinement " may be defined as an irregular junction which marks a break in sedimentation. The break may be due to a period of denudation consequent on movements of masses of water, but not necessarily accompanied by earth-movements. Although one of the commonest of geological phenomena, there is no English word which expresses quite so aptly the relationship. One may say that there is an " unconformity " at the base of the marine formation, but that term rather suggests discordance of stratification and earth-movements of a more pronounced character. The term " plane of erosion " is bad, since the basal conglomerate tends to occupy an irregular or hummocky surface of the underlying deposits rather than a plane surface. " Non-sequence " is most frequently applied to a break in sedimentation which can be detected only by a study of successive faunas.

are given in Fig. 1. One sees immediately the comparative duration of the cycles, the relative duration of the marine and continental periods in each and the approximate depth of formation of the deposits. By comparison of two or more graphs, using well-marked

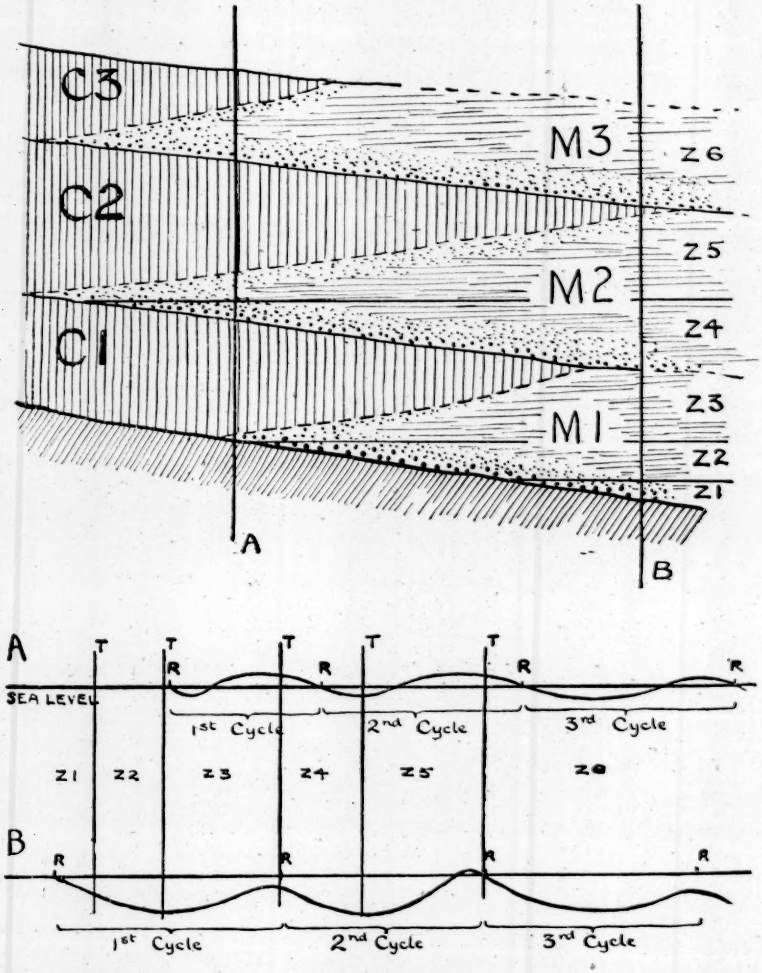


FIG. 1.—Diagrammatic representation of three cycles of sedimentation. M = marine deposits, showing gradation in coarseness. C = continental deposits. Z = zones, as determined by palæontology. The two graphs show the oscillations of the land surface relative to sea-level at the points A and B. R = position of "ravinements". T = junction planes between zones, used as time planes to show the relative time of commencement of each cycle at the points A and B.

palæontological horizons as time planes, one can trace the relative time of commencement of each cycle in different places.

In an enclosed or partly enclosed basin it is not difficult to see that intermittent earth movements would cause oscillations in the water-level and give rise to a series of cycles of sedimentation. As will be seen later, this is precisely the condition of formation of the Eocene strata of the Anglo-Franco-Belgian region. Each movement caused a marine transgression, which was followed naturally by silting up and retreat of the sea.

In a broad sense and on a large scale the importance of the sedimentary cycle has long been recognized. It is the *raison d'être* of most of the systems before they received more exact palæontological definition. Professor Watts, in his Presidental Address to the Geological Society in 1911,[1] has dealt exhaustively with the sequence of deposition. Commencing with abysmal deposits he has traced the changes through shallow-water and littoral sediments to beds deposited during emergence, then the beds formed during a succeeding submergence, passing finally back to abysmal deposits. This is a simple way of presenting the sequence, but it must be borne in mind that the natural period commences with the unconformity at the base of the marine beds (i.e. coinciding with a submergence of the land), and the abysmal deposits occupy the middle period of the cycle, its termination being marked by the continental deposits and finally abruptly limited by the succeeding marine invasion.

The application of these principles to detailed classification on a small scale has been given primary consideration on the Continent. One may quote the work of the Geological Survey of Belgium. The nomenclature and grouping used on the maps indicate at once the importance attached to it. Each formation is given an index letter, and the deposits in each formation are marked by a second letter. Taking, for example, the Ypresian (London Clay) Series, one has the following divisions :—[2]

Ye. Littoral deposits (" gravier d'émersion ").
Yd. Sand indicating shallowing conditions.
Yc. Clay (main mass).
Yb. Sand of basement bed.
Ya. Basal conglomerate (" gravier d'immersion ").

The bed " e " in most cycles is removed before the deposition of succeeding strata. In the Belgian classification the continental deposits generally receive a separate index letter. In some cases, however, a bed which does not fit in the main series is given the distinguishing letter " m " or " n ", etc. For example, at the base of the " Paniselian " there is a stiff blue unfossiliferous clay,

[1] Watts, " Geology as Geographical Evolution " : *Quart. Journ. Geol. Soc.*, vol. lxvii, 1911, p. lxii.

[2] " Légende de la Carte géologique de Belgique . . . à l'Echelle du 40,000e ", 1891, etc. : Review in *Ann. Soc. géol. Belg.*, vol. xix, 1891, p. 107.

frequently regarded as of lagoon origin; this is called *P*1*m*, the Paniselian having been divided into two cycles, *P*1 and *P*2. It seems probable that this scheme of nomenclature has been carried too far by the Belgian Survey, for example in the Devonian, where the artificial grouping of the deposits of each division (Coblentzian, etc.) has obscured the more or less continuous marine transgression of Lower Devonian times. However, even there it has the advantage of giving some indication of the lithology of the beds by a single symbol.

The application of the principle to the classification of the Eocene strata of the Paris Basin is due especially to Professor Leriche.[1] As will be seen later these deposits are especially suitable for classification in this way.

II. THE ANGLO-FRANCO-BELGIAN BASIN.

The Eocene rocks of the area under consideration occur in four principal districts—the so-called "Basins" of Paris, Northern France and Belgium, London, and Hampshire. Separating these four areas of Eocene rocks there are uplands of Cretaceous or older rocks. It is now quite certain that they are not original basins of deposition, but formed part of one great basin—the Anglo-Franco-Belgian Basin. The folding which has affected this extensive area and caused its separation into the present "basins" took place, for the most part, in post-Eocene times. One finds, for example, in the monocline of the Isle of Wight [2] and the anticline of the Pays de Bray [3] that the Eocene beds are involved in the movements. Their thickness is quite independent of the folding. Hence it may be inferred that the movements did not *commence* until after Eocene times. On the other hand, there is a large amount of evidence that the uplift of the Wealden Dome commenced even before Eocene times, and went on gradually throughout the early Tertiary period. The effects of the movement can be clearly seen in the character of the Eocene deposits of the London Basin. The areas of uplift between the present "basins" have been stripped by denudation of their Eocene rocks, and only a few scattered outliers remain to indicate the former continuity of the deposits.

The Paris Basin is situated mainly to the north of Paris. Southwards the Eocene series becomes incomplete, since some of the marine invasions did not penetrate beyond the latitude of Paris. The Paris Basin is separated from the Eocene region of Northern France and Belgium by the Chalk uplands of Artois, Picardy, and Champagne, and one only finds a few patches of Lower Eocene rocks from the

[1] Leriche, "Observations sur la Classification des assises paléocènes et éocènes du Bassin de Paris": *Ann. Soc. géol. Nord*, vol. xxxiv, 1905, p. 383.

[2] Bristow, *Geology of the Isle of Wight* (Mem. Geol. Surv.), 2nd ed., 1889, p. 242.

[3] Dollfus, "Essai sur la détermination de l'âge du soulèvement du Pays de Bray": *Bull. Soc. géol. France*, sér. III, vol. ix, 1881, p. 112.

neighbourhood of Laon to the neighbourhood of Lille. In the drift and clay-with-flints which partly covers the Chalk, however, Leriche has found fossiliferous remanié blocks proving the former presence of all the Eocene deposits.[1]

In Belgium Eocene rocks occupy the whole of the northern and western parts of the country, and this "basin" extends into the adjoining regions of Northern France. It stretches northwards into Holland, but the Eocene rocks are there covered by a great thickness of later deposits. To the south-east the Ardennes apparently formed an area of dry land during most of the Eocene period, and the deposits tend to assume a littoral character as they are traced in this direction.

On the extreme west of the North France–Belgium region, near Calais and St. Omer, the succession is so similar to that on the opposite side of the Straits of Dover in the Isle of Thanet and near Herne Bay that there seems little doubt concerning the former continuity of the deposits. One sees, resting on the Chalk, the glauconitic marine Thanet Sands (Landenian) with harder beds ("tuffeau") in the lower part and the "Bullhead Bed" of green-coated, unrolled flints at the base, both near Ramsgate and St. Omer. The succeeding Ypresian commences at Reculvers (Herne Bay)[2] with a pebble-bed and about 20 feet of very fine brownish sand (Oldhaven Sand), followed by the great mass of stiff blue clay (London Clay). Around Calais[3] there is the same succession—first the pebble-bed, then the bed of very fine brownish sand, and then the great mass of London Clay (Argile yprésienne). Moreover, the faunal evidence corroborates the lithological.

The London Basin is too well-known to need any introductory remarks. There is every reason to believe that the Hampshire Basin was originally joined to the extreme western end of the London Basin. The existence of a few small outliers as well as relict blocks ("sarsens") on the intervening Chalk lands and the close lithological resemblance between the two areas are sufficient proof. The central portion of the Wealden Dome probably formed an island of low relief or a submerged area within the reach of wave action during the greater part of Eocene times.

The outliers at the eastern end of the Hampshire Basin, especially at Newhaven, form a connecting link with the small outliers on the other side of the Channel near Dieppe. In many respects the Eocene beds of Dieppe resemble the Hampshire deposits more clearly than they do the Paris deposits. Not only are the Dieppe deposits

[1] Leriche, *Ann. Soc. géol. Nord*, vol. xxxii, 1903, pp. 120, 239; vol. xxxviii, 1909, pp. 74, 421. C.r. Ass. Franç. Avan. Sci., Cherbourg, 1905, p. 394; ibid., Lille, 1909, p. 408. *Bull. Soc. géol. France*, sér. iv, vol. xii, 1912, p. 690 et seq.

[2] Whitaker, *Geology of the London Basin* (Mem. Geol Surv.), vol. iv, 1872, p. 170, etc.

[3] Briquet, "Observations sur la Composition des Terrains Eocènes inférieurs du Nord de la France": *Ann. Soc. géol. Nord*, vol. xxxv, 1906, p. 132.

connected with the main part of the Paris Basin by a large number of outliers, but along the shores of the English Channel there are other outliers (at St. Valery-sur-Somme, Montreuil, etc.), forming a connecting chain with the deposits of Northern France.

Summarizing the geographical conditions in the whole region at the time of formation of the Eocene strata, there was a single large basin stretching from Norfolk on the north to beyond Paris on the south, and from Devonshire on the west to the Limburg (Eastern Belgium) on the east. This basin may have been enclosed, but was more probably open to the north-east. Sediment was being poured into it from two principal sources—by a great river flowing eastwards into the basin in the London and Hampshire regions, and by another river emptying northwards in the Paris region. Subsidiary sources of sediment probably lay to the east in Belgium. It is hoped that a study of the detrital minerals, at present being carried out by the author, will throw further light on this subject. Throughout the period the basin was subjected to earth-movements, of which the gradual but intermittent uplift of the Wealden Dome seems almost certainly to have been one result. This caused a periodic transgression and regression of the marine waters of the basin, and so it is only in the centre that one would expect to find a succession of marine deposits. Round the periphery one should have, if conditions for preservation were ideal, a succession of continental deposits. Over the greater part of the whole region one has a succession of marine and continental deposits in regular alternation, and the beds can be grouped naturally into a series of cycles of sedimentation. As will be shown later, the same cycles are traceable over the whole basin, and it is essential to consider the four areas—France, Belgium, London, and Hampshire—as a single region.

(*To be continued.*)

The Relative Age of Concretions.

By W. Alfred Richardson, M.Sc., B.Sc. (Eng.), F.G.S.

I. The Relative Age.

THE age of a concretion, relative to that of the parent-rock, must be either :—

1. *Contemporaneous.*—The concretion grew on the sea-floor at the same time, and at about the same rate, as the surrounding deposit was laid down.

2. *Penecontemporaneous.*—The concretion segregated more or less close to the surface of recently deposited sediment.

3. *Subsequent.*—The concretion formed after deposition had finished, and after the strata had largely consolidated, but, perhaps, before cementation had taken place to any noteworthy extent.

Further, if the concretion were formed subsequently to the main deposit its attitude to the mother-rock must have been either :—

(*a*) *Displacive*. The original material has been pushed aside to make room for the later.

Or (*b*) *Replacive*. The original material has been carried away in solution, and the new deposited progressively in its place.

There is also the possibility that a replacive concretion may cause some displacement of the strata, if its volume be greater than that of the original material.

Workers who wish to determine the relative age of segregations appeal mainly to facts gathered by field and microscopic study. Yet it is safe to say there is hardly a class of fact that has not been adopted by advocates of both contemporaneous and subsequent origin in support of their very different contentions. It is surprising on this account that no attempt to examine critically the significance of fundamental tests of relative age appears to have been made.

II. Bending of Lamination Planes.

The bending of planes of lamination so that they conform to the shape of a concretion is often reported. Some observers consider this feature a proof of the secondary displacement of the strata ;[1] whilst others see in it only evidence of the resistance of an object, harder than the main deposit, to the weight of overlying material.[2]

On reflection, however, it will be seen that there are three distinct ways by which such a structure may have arisen, namely :—

1. Accumulation of sediment in layers over a contemporaneous segregation.
2. Differential consolidation of the sediment in the neighbourhood of a contemporaneous segregation.
3. Progressive displacement of rock-layers by a subsequent concretion.

The effect of sedimentation may be considered before the other possibilities. The arrangement of successive layers with respect to an object already on the sea-floor will be determined largely by the following factors :—

1. Gravitation.
2. The angle of repose of the material—a measure of its internal friction in water.
3. The sensitiveness of the deposit to water-currents.
4. Friction between the object and the deposit.

The last is doubtless negligible as its influence is theoretically limited to a layer one particle thick, and beyond only the internal friction between particles of the deposit comes into play.

The general operation of these factors was made the object of simple experiments. A rectangular glass vessel was provided with a plasticene floor, the slope of which to the horizontal could be varied readily by tilting the vessel. Two types of material were used, and the results may be summarized thus :—

[1] Dean, *Amer. Journ. Sci.*, vol. xlv, 1918, p. 411.
[2] Gardner, ibid., vol. xvi, 1908, p. 457.

1. A flocculent precipitate, obtained by the addition of ammonia water to a solution of an aluminium salt, had a very low angle of repose, but settled in still water along the flatter slopes. The slightest movement of the water, such as that due to a gentle tapping of the vessel, caused immediate settlement of the precipitate to a level surface.

2. Fine silver sand had a greater angle of repose, and also required a much stronger water-movement to level it.

3. There was no tendency for either of these to adhere to the plasticene surface. When the deposit accumulated along the slope in still water the layer as might be expected was thicker at the base.

Snow may be frequently seen to heap up over objects. But it does so permanently only when partial melting and refreezing have welded it into something like a continuous sheet. A slight breeze is sufficient to blow dry, powdery snow from elevated objects, and it then settles into drifts, the shape of which is determined chiefly by wind-eddies and the protection afforded by surrounding objects.

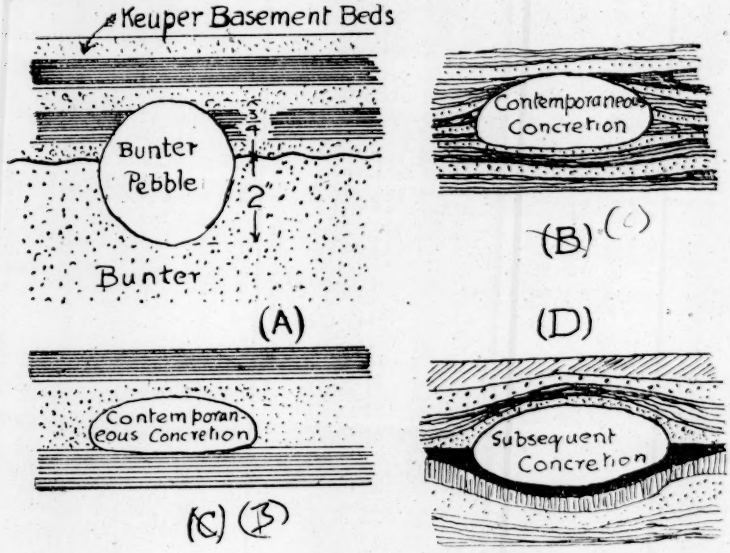


FIG. 1.—(A) Type of sedimentation around a contemporaneous object; laminæ thinner than object. (B) Similar to (A), but laminæ thicker than object. (C) Vertical loading on contemporaneous concretion producing "catenary" bedding. (D) Conformity of bedding planes to subsequent concretion, produced by displacement during growth.

In any case the density and viscosity of such a medium as water would, when in motion, be more effective than air in bringing loose material to a gravitational surface. Ample confirmation of this is afforded by the examination of stream beds, pools, and shore margins. Boulders and other objects stand clear even of a sandy deposit, for,

although possessing a large angle of repose, sand occurs in zones of relatively strong movement

A further example is afforded from the Nottingham district, where pebbles frequently project from the wind-eroded surface of the Bunter into the overlying, laminated, Keuper sands.[1] Professor H. H. Swinnerton kindly allows me to publish his field sketch of one case (Fig. 1*a*). It will be seen that the laminæ are level and terminate abruptly at the pebble, showing no tendency to heap over it.

Experiment and observation, then, seem to indicate that accumulation of sediment in layers, conformable in shape to a contemporaneous nodule, is unlikely to be frequent. However, such an origin of the structure admits of ready determination, for conformity would be confined to laminæ passing above a concretion —those below necessarily being unaffected. I consider, on the whole, that the characteristic arrangement would be for thin layers to terminate abruptly against a nodule (Fig. 1*a*), and for those thicker than the nodule to pass in horizontal lines above it (Fig. 1*b*).

Before considering the remaining possibilities, it may be mentioned that conformity of stratification lines to the shape of a nodule has usually, if not invariably, concomitant phenomena. Such are smooth or slicken-sided contacts ; a tendency to the development of cone-in-cone structure in the surrounding matrix ; and miniature faulting. These may be fairly attributed to pressure, but whether the pressure is generated by nodular growth or vertical loading has not been established.

These two cases were tested experimentally in the way indicated below :—

(*a*) A cake of hard soap was embedded in a matrix, formed by superposed layers of different coloured plasticene. These were arranged so that they were thinner than the cake, and terminated, without initial bending, abruptly at its surface. The model was subjected to vertical pressure in a hydraulic machine. When removed from the press and sectioned vertically, the laminæ were found to be compressed over the nodule, thinning out above and below, and generally conformable to the shape of the cake. The removal of the cake showed that smoothing of the contact had taken place (Fig. 1*c*).

Therefore, pressure applied vertically to a contemporaneous nodule would be competent to cause bending of stratification lines.

(*b*) The attempt to imitate a nodule "growing" in a deposit presented difficulties. Finally it was decided to force a steel ball gradually into a layer of coloured plasticene. Suitable arrangements were made to prevent plasticene from squeezing up around the ball, as the latter was forced very slowly into the model. Since the ball thus entered at a point, and the volume of the intrusion increased

[1] Swinnerton, *Proc. Geol. Assoc.*, vol. xxix, 1918, p. 20.

with the penetration, the action may be considered as a reasonable imitation of the mechanical conditions that prevail as a nodule grows gradually into laminated stratum.

The lamination planes were seen when the model was sectioned to be bent conformably to the shape of the ball (Fig. 1*d*). Therefore, a concretion exerting pressure on a medium by gradually forcing itself outwards during growth is also competent to produce conformity of stratification lines to its own shape.

Now, comparing these two cases from the mechanical standpoint, they are related inasmuch as the generating force of the one is the reaction in the other. The general tendency will be to compress layers of the matrix above and below, causing a sideway flow of material into interspaces between the nodules. The net result in both cases is bending of the layers, with vertical thinning and the simultaneous production of subsidiary pressure effects.

It is now essential to determine any features that may be used to distinguish between the two cases. The foregoing discussion has already made it probable that with contemporaneous origin of the concretion the layers will probably settle to a gravitational surface around the nodule. If these layers are thin compared with the nodule, they may be expected to terminate abruptly against its sides, and under the influence of pressure, as in the model, will wedge upwards against the side of the nodule, after the manner of "catenary" bedding (Fig. 1*c*). If, on the other hand, the deposit is thicker bedded it is difficult to see how the question of relative age could be determined from the phenomenon itself. It may be remarked that if consolidation pressure produced this effect, one might expect it to be at least as frequent over undoubted contemporaneous objects, such as fossils, as it is over nodules. But it certainly is not. Indeed, I have sought long for something of this kind in the field (and I am indebted to many others who have also kept watch in their field work for the same evidence), but hitherto without success. Moreover, there does not appear to be a recorded case. This is on the whole rather unexpected, and I am gradually coming to the opinion that conformity of stratification lines is caused in every case by concretionary growth, and that the date of formation is at the earliest later than the first compacting of the matrix.

III. Replacement.

Replacement of fossils, of the grain and structural features of the original rock has always been regarded as undoubted evidence of secondary origin. An excellent application of the study of progressive replacement to the problem of relative age may be found in Miss Raisin's [1] investigation of the Portland Cherts.

W. A. Tarr,[2] however, has suggested a different explanation of

[1] Raisin, *Proc. Geol. Assoc.*, vol. xviii, 1903, p. 75.
[2] Tarr, *Amer. Journ. Sci.*, vol. xliv, 1917, p. 428.

fossiliferous chert. On his hypothesis of chert formation, nodules originally existed as globular segregations of silica jelly on the sea-floor. The fossiliferous character of the cherts is explained as due to the infalling of animal remains, which accumulated in these silica balls in the same way as in the surrounding rock.

This hypothesis presents mechanical difficulties. A few experiments with any gel, even though it may contain a large amount of water, will suffice to show that, as soon as a gel has sufficiently coagulated to stand under its own weight, it offers considerable resistance to surface penetration. Heavy objects may be left indefinitely on its surface, and penetrate no further than a small initial indentation.

Large heavy shells possess considerable surface, and this would prevent their penetration into the gel. With regard to minute forms, such as the tests of foraminifera, it is impossible to conceive how *they* could have penetrated the gel. They must also arrange themselves in the same way as they are to be found in the surrounding rock. Yet the viscosity of the gel, even though one granted the possibility of penetration, would exercise a considerable effect on the arrangement of the contents.

From such considerations it would seem that our faith in the old argument of replacement is not considerably weakened by this ingenious suggestion.

IV. Modern Occurrence.

The occurrence, or non-occurrence, of a particular type of segregation in modern seas has often been employed as a subordinate criterion of relative age. It is, of course, an argument based on comparison. Certain types of concretion, e.g. manganese nodules and phosphatic segregations, *are* found. It, therefore, seems reasonable to expect careful search to reveal something of other concretions that are as abundant or more abundant in rocks of various geological ages.

The argument has been frequently opposed. Sometimes on the ground that the specific gravity of the segregation would carry it deep into the deposits of the ocean-floor beyond reach. However, the list of the materials dredged in the numerous expeditions disposes of specific gravity as the cause of non-discovery. The list includes manganese and phosphate nodules; barytes, terrestrial rocks, including detrital flints, heavy animal remains, debris dropped by steamers, and meteoric granules.[1]

A more serious and less answerable objection lies in the possibility that physical conditions necessary for formation of the concretion in question do not pertain in the present ocean. The argument is, therefore, quite secondary, and must be used with due caution. It obtains weight only when a considerable body of other evidence points the same way.

[1] Murray & Hjort, *The Depths of the Ocean*, London, 1912, chap. iv.

V. The Shape of Concretions.

H. C. Sorby [1] considered that the shape of nodules depended on that of their nuclei, and gave a series of measurements to demonstrate the point. In some cases this dependence is doubtless a fact, but a broad survey of concretions does not warrant a general application of the principle. Some examples will be given in the following paragraphs to illustrate the subject.

L. Moisey [2] examined several thousand ironstone nodules from the Notts–Derby coalfield. These nodules, containing fossils flattened vertically, are, on the whole, small, and ellipsoidal in shape. The length and depth of the nodules corresponds fairly well with the length and depth of included fossils, but their width seems independent of that of the fossil. Moreover, some 35 per cent of the nodules examined contained no fossil, yet were similar in shape to those that did.

I have gone over the tip-heaps and exposures afforded by quarries in the Lower Lias of Lincolnshire to collect evidence of the influence of inclusions in nodules. A few of these Liassic doggers (often septarian) may have a large ammonite at their centre. In the majority either no fossils were found, or the fossils were distributed sporadically through the nodules. The general shape of these nodules was also a flattened elipsoid.

A colleague, Mr. H. S. Holden, recently brought me a small concretion in Coal-measure shale from Gedling. The concretion formed in the neighbourhood of a *Carbonicola*, and its final stages of deposition just embraced the tip of the shell. This small concretion was almost spherical, and the shell had evidently no influence on the origin or location of the segregation. The lamination planes were disturbed by the concretion, but not by the fossil, thus indicating a secondary origin.

Chalk flints occasionally show in their shape the influence of their contents. The majority are, however, highly irregular. This irregularity might be due to local differences in the Chalk, that enabled replacement to take place more easily in certain directions than in others.

Most concretions that have been dredged from the ocean appear to form around some object. And if they are comparable in size with the nucleus, their shape seems to be strongly influenced by it.

Liesegang [3] emphasized the general tendency of concretions to assume a globular form. He demonstrated partly by experiment and partly by field observations, that if the segregation be forced initially to depart from the circular form by commencing at an irregular centre, the concretion tends to approximate more and more to that shape as growth progresses.

[1] Sorby, *Quart. Journ. Geol. Soc.*, vol. lxiv, 1908, p. 216.
[2] Moisey, Geol. Mag., Dec. V, Vol. V, 1908, p. 222.
[3] Liesegang, *Geologische Diffusionen*, Dresden and Leipzig, 1913, chap. vi.

Large numbers of concretions—in fact one may safely say the greater number of concretions—tend to have a shape that may be described as an ellipsoid flattened vertically. This shape may appear indifferently, whether there are central fossils or not. The form suggests a possible analogy with the ellipse of stress, or the ellipse of optic elasticity. If an isotropic substance is compressed in one direction the ease of vibration of a ray in that direction is reduced, and, as for uniaxial crystals, the distribution of vibration ease can now be represented by an ellipsoid of rotation.

It seems reasonable to suppose that concretionary growth should be similarly influenced by the state of stress of the medium. In a homogeneous and unstressed matrix growth should proceed at equal rates in all directions, and, therefore, in a given time reach a spherical boundary. On the other hand, a concretion forming subsequently to the deposition of a rock, and located at some distance from the surface, encounters considerable resistance to growth in a vertical direction. The rate of increase in this direction should, therefore, be less than it is horizontally, and the flattened ellipsoidal shape that prevails in many concretions would be explained.

This phenomenon can be demonstrated by carrying out Liesegang's experiment in a gel subject to greater compression in one direction than in another. It is rather difficult to be sure of getting an unequally stressed gel, but the method described below was successful.

Pour a bichromate gel into a cardboard tray to a depth of about $\frac{1}{8}$ in. Support a glass, or xylonite plate, provided with a small central hole, in contact with the surface of the gel until the latter has completely set. The transparent plate must fit the tray *exactly*. When setting is complete, cut away two opposite sides of the tray, supporting the other pair by wooden blocks. Press the plate on to the surface of the gel and apply weights. The gel is now free to move in one direction, but hindered in the other. It is, therefore, compressed against the remaining sides of the tray. Now place a drop of strong silver nitrate over the central hole, and leave in a damp atmosphere to diffuse.

In all experiments where differential straining of the gel was successful the diffusing silver nitrate reached an elliptical boundary. The shape obtained in two experiments, with the dimensions of the axes, is given in Fig. 2.

It should be mentioned that J. Geikie[1] considered that the horizontal extension of a concretion is due to the greater ease of diffusion provided by lamination planes. This would no doubt assist the phenomenon, but cannot be considered as a major cause, since precisely similar ellipsoids are found in more massive beds, and not located along bedding planes.

[1] Geikie, *Structural and Field Geology*, Edinburgh, 1912, p. 121.

It will, therefore, be seen that contemporaneous concretions would be favourably placed for the development of spherical form. Examples of such are manganese nodules and " marlekor ". The latter are aggregations of spheres and have been imitated in unstressed gels by Liesegang.[1] The spherical shape may also be attained by concretions of subsequent age, providing there is a homogeneous environment. On the other hand, the flattened ellipsoidal shape seems confined to displacive, or partially displacive subsequent nodules. Replacements are capable of very irregular development as many economic deposits show.

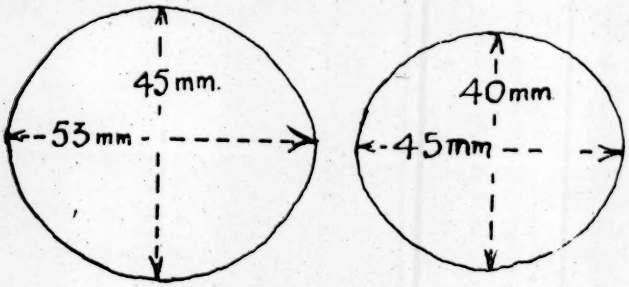


FIG. 2.—Shape and dimensions of two boundaries reached by silver nitrate diffusing in an unequally stressed gel.

VI. THE DISTRIBUTION OF NODULES.

The following modes of distribution may be distinguished :—

1. Sporadic distribution appears to be characteristic of most concretions to be found on the ocean-floor or in recent alluvial deposits.

2. The majority of phosphatic nodules are located at a definite level. The nodules themselves are frequently rolled, bored by molluscs, and carry attached contemporaneous animal remains. They are contemporaneous and have been formed where there is a pause in sedimentation.

It may be remarked in this connexion that so far as the evidence goes, modern concretions seem to form very slowly. It follows that they must occur where sedimentation is also slow.

3. Some nodules occur in definite lines, parallel to the bedding. The spacing of the lines follows apparently some law or " rhythm ", sometimes in a very marked way. Moreover, several observers have called attention to a tendency for the distance between the individual nodules of a given bed to be uniform. Failures to find competent external causes of this arrangement have led to the suggestion that the rhythmic precipitation of diffusing solutions is the cause. In some cases there is a strong resemblance to the sequence of Liesegang banding. For example, closely spaced bands may be followed by a region where the bands are progressively more

[1] Liesegang, loc. cit., chap. xviii.

widely spaced, as indicated in Fig. 3*a*. The close regular banding may be both followed and preceded by such regions (Fig. 3*b*). Widening of the spacing is often accompanied by a reduction in the average size and number of nodules, and by the tendency for the banding to give way to " scattered zones ".

The whole series may represent a single rhythm. There is, then, a presumptive case that all the concretions were formed by one process of precipitation. As diffusion continued the concretionary layers were deposited progressively—the whole process extending over a period, as to the duration of which nothing is known.

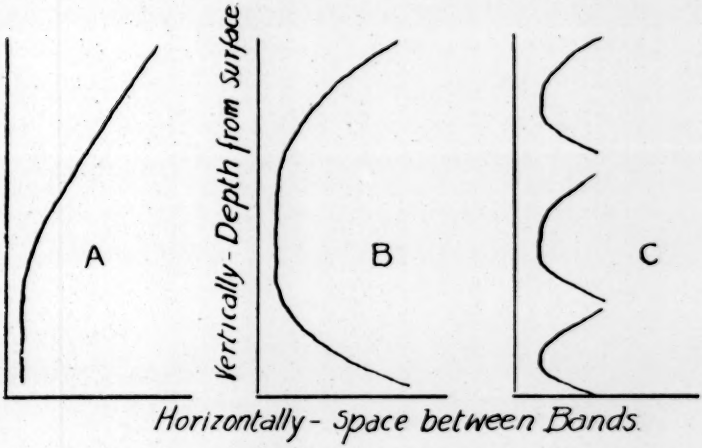


FIG. 3.—A, Liesegang rhythm. B, Another type of Liesegang rhythm. C, Three Liesegang rhythms in one deposit.

On the other hand, it may sometimes be possible to divide up the layers of a given deposit into sets, each showing the characteristics of a single rhythm. The case is presented diagrammatically in Fig. 3*c*.

If only a single rhythm can be detected it may be taken that the concretions are the result of a single process spread over a time-interval commencing at the earliest after the complete deposition of the rock.

The detection of two or more such rhythms in a single series indicates that after the overburden of sediment had reached a certain thickness precipitation and concretionary growth took place. Such may be the conditions under which concretionary liassic limestones were deposited according to the hypothesis of W. D. Lang.[1] This hypothesis has recently been further investigated by E. F. Davis,[2] and called by him " colloidal segregation ".

[1] Lang, *Proc. Geol. Assoc.*, vol. xxvi, 1915, p. 117.

[2] Davis, *Bull. Dept. Geol. California Univ.*, vol. LL, vol. II, vol. 11, No. 3, 1918, p. 394.

VII. Summary.

The following features are favourable to contemporaneous age :—

1. Spherical form.
2. Sporadic distribution or occurrence at a pause in sedimentation.
3. Gravitational bedding around the nodule.
4. Modern occurrence.

The following features are favourable to the subsequent relative age :—

1. Replacement of the fossils, grain, and structural features of the parent rock.
2. Displacement of the stratification lines, above and below the nodule, conformably to its shape.
3. The development of a vertically flattened form.
4. The arrangement of the nodules in lines showing a marked rhythm.

It should be hardly necessary to remark that no one of these features can be decisive. The whole of available evidence must be taken into account, and considered with any special local evidence that may be available. Thus, G. W. Lamplugh's[1] observation that a band of crushed ammonites passes through the nodules puts the date of the concretion as later than the crushing. The subject is difficult and agreement is far from being reached, although the matter has received the attention of generations of able geologists.

I should like to express my thanks to Professor H. H. Swinnerton, D.Sc., and to Messrs H. S. Holden, M.Sc., and H. C. Sargent, F.G.S., for discussing various aspects of this work with me.

On the Geological Features of the Oil Region in the Northern Punjab (British India).

By H. Preiswerk, Basle.

(*Concluded from p. 80.*)

Occurrence of the Oil.—The oilfield of Jaba and those of Rawalpindi and Fatehjang are identical from the stratigraphical point of view. At Jaba, too, the oil is to be found in the contact zone between the Nummulitic limestone and the younger gypsiferous clays of the Upper Nummulitic. These gypsiferous clays represent a bed well adapted to favour the accumulation of oil, wherever they cover the limestone at a moderate inclination. The oil issues only from the limestone, but the cellular limestone, too, may be oil-bearing in depth. It may be pointed out that cellular limestones of the same character accompany the oil-shows at Gunda, the main oil-spring in the neighbourhood of Fatehjang.

At Chota Kutta and Burra Kutta the oil flows out mixed with

[1] Lamplugh, *Quart. Journ. Geol. Soc.*, vol. xiv, 1889, p. 582.

sulphur water, but in the boring at Chota Kutta the oil is free from water as far as I know.

Near the oil-springs the beds have steep inclinations, especially at Burra Kutta. In order to find more favourable places for borings we have to discover localities where the roof of Nummulitic limestone lies under the ground and almost flat. My geological researches in this part of the Salt Range show that such places probably exist. First we may suppose that the oil-bearing beds take on in depth a flatter dip, as the overlying sandstone beds show a lower dip to the north-east only a short distance from the range.

Of importance are also those places where the axis of an anticline pitches to such a degree that the core of the fold, which is formed by Nummulitic limestone, is covered by the Upper Nummulitic and the Murree beds. Such places are found south of Burra Kutta, where the anticline A dips below, and at the entrance of the ravine of the Majukh river, where the anticline B comes to an end on the surface.

Extension of the Oil-bearing Beds in the Salt Range.—It is quite probable that the occurrence of oil is not limited to the immediate neighbourhood of the oil-springs, but has a larger extension to the north-west and south-east along the border of the range. The geological features of this border ought to be studied more completely. Several occurrences of tar south-east of Jaba are indications of the extension of the oil-bearing beds over a large part of the western Salt Range.

At Kallar Kahar I have obtained a sample of tar which was brought by a native from the outcrop 2 miles north of Khabakki in the Shahpur District. According to A. B. Wynne's excellent geological map, these outcrops are situated near the upper limit of the Nummulitic limestone, that is to say, on the oil-horizon. Accordingly the tar occurrence at Khabakki acquires some importance as an indication of the extension of oil rocks in the western Salt Range. B. S. Lyman mentions in his report several tar-occurrences in this district: "The Chinnoor rock tar near Murdowal (Shahpur District), and 83 miles south-west of Pindi; the Hungooch rock tar near Dhuddo and Murkoom (Shahpur District), and 81 miles south-west by south of Pindi; and the Doona rock tar near Kubbukkee (Shahpur District), and 77 miles south-west by south of Pindi." There is no doubt that Lyman's "Kubbukkee" and the "Khabakki" of the topographical map (scale 1 inch = 1 mile) are the same. The names "Murdowal", "Dhuddo", and "Murkoom" are probably to be identified with the names "Mardwal", "Dadhar", and "Mukrumi" of the topographical map (No. 43 D 2). Mardwal is situated 5 miles, Dadhar and Mukrumi 3 miles west of Khabakki. These distances might be brought in accordance with those given by Lyman.

Evidently we have to deal here with several tar outcrops, running with the strike of the strata for many miles, a fact which is worthy of consideration.

In the eastern Salt Range, however, as before shown, we find quite another petrographic character of the Upper Nummulitic. Near Kallar Kahar the coarse oolites—the same as those that form the roof of the oil-beds in Jaba—are also developed, but they lie directly on the massive Nummulitic limestone. The cellular limestones and the gypsum marls are missing. Farther east the Upper Nummulitic, developed as yellow brittle marls and calcareous nummulitic beds, is only about a metre thick. There are no oil-traces to be found in the whole eastern Salt Range in this horizon.

C. THE BURNING GASES OF JAWALAMUKHI, DISTRICT KANGRA.

Jawalamukhi may be reached from the railway stations Hoshiarpur or Pathankot. A new and well-made road, suitable for tongas, leads from Pathankot to Kangra (53 miles). From Kangra another road leads to Jawalamukhi (23 miles); this can be used, however, by ekkas only.

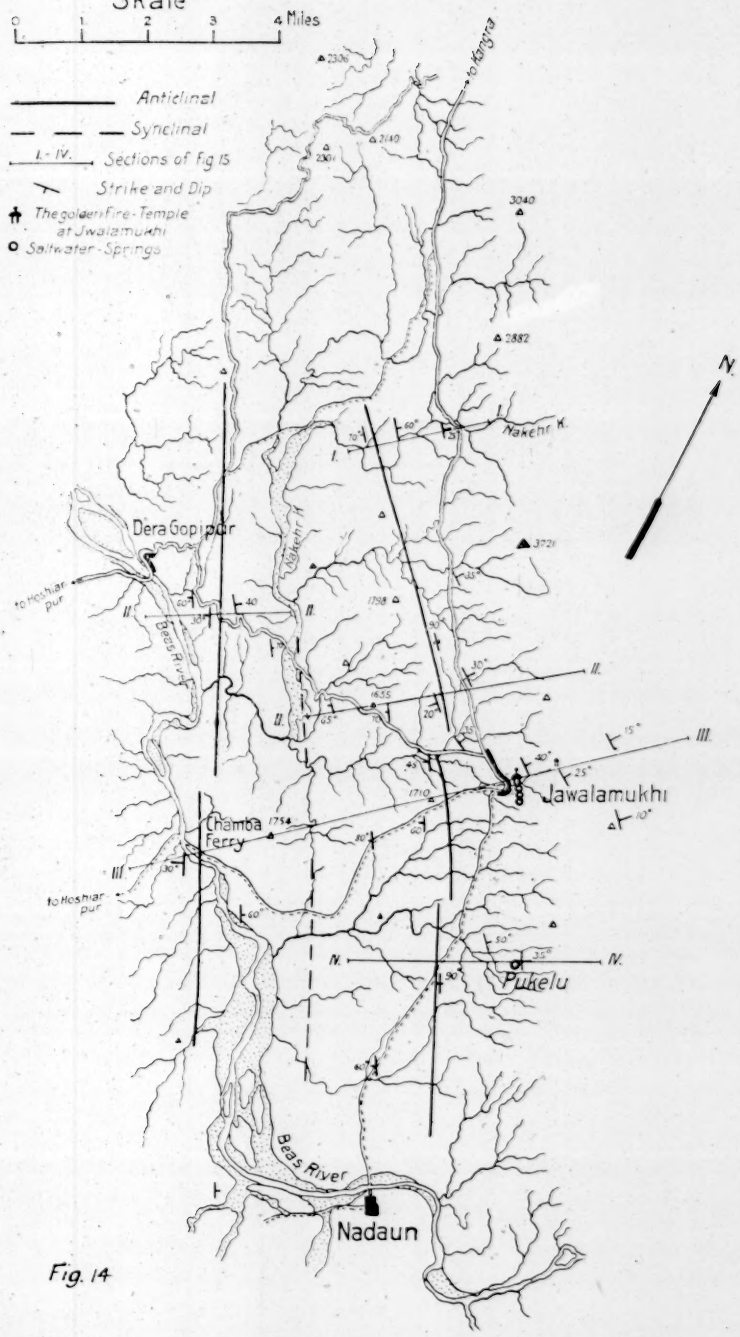
In the temple, which is often visited by Hindus, inflammable gases rise from the ground. In the chief room of the temple visitors are shown several feebly luminous, bluish flames bursting out of a hollow. In another part of the temple, lying a little higher, about 10 metres distant from the former, the gas comes out of a water reservoir, and is brought to explosion from time to time. The colours of the flames lead one to think that the gases chiefly consist of methane.

H. B. Medlicott (Mem. Geol. Surv. India, vol. iii, 1864, p. 146) mentions this occurrence in the following words: "At the famous shrine of Jawalamukhi an inflammable gas perpetually issues from the rock (unless, indeed, it be cooked by the priests below ground)". My examinations in the field, however, proved remarkable relations between the geological features on the one hand and the occurrence of the gases on the other, so that Medlicott's suspicions may be without foundation.

In the brook behind the fire-temple there is a bluish-gray sandstone bed about 15 metres thick, covered by red marls dipping north-east at somewhat more than 40°. On the right side of the brook the red marls and the sandstone bed lie about 20 metres lower. They have sunk along a small transverse fault running north-east. Above the sandstone bed there follow with frequent alternations sandstones with fossil plants and red marls with calcareous concretions. Similar rocks form the whole steep slope (over 1,000 feet high) to the east of the village. The marly layers, however, decrease a little towards the top, sandstones predominating.

Towards the top the dip of the beds decreases continually. On the crest of the mountain they dip only at 25° north-east and farther east 15° and less.

According to Medlicott this series and all the other rocks of the environs of Jawalamukhi belong to the Siwalik formation.


Skale
0 1 2 3 4 Miles
Anticlinal
Synclinal
I.- IV. Sections of Fig 15
Strike and Dip
The golden Fire-Temple at Jwalamukhi
Saltwater-Springs
to Kangra
2306
2140
2301
3040
2882
3721
1758
1655
1710
1754
Nakeni R.
Dera Gopipur
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Beas River
Chamba Ferry
Jawalamukhi
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Nadaun
N.

Fig. 14

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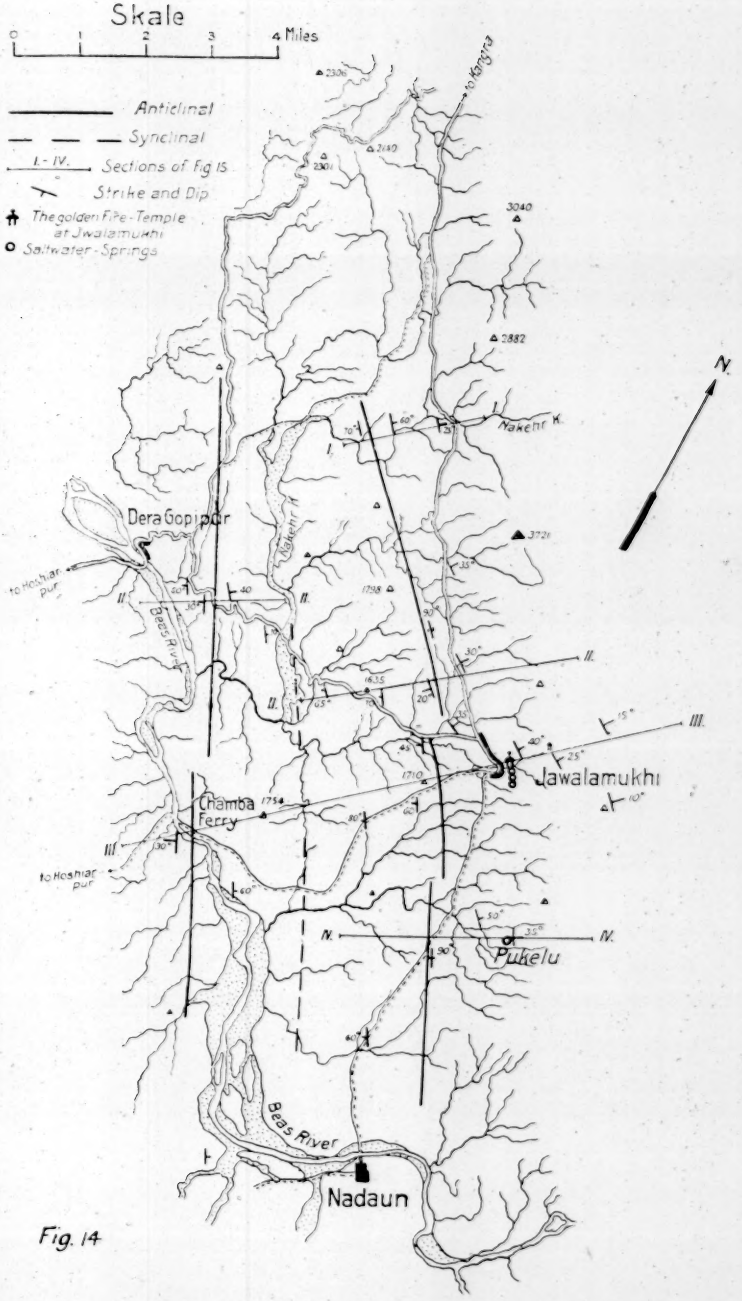
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Jawalamukhi
Chamba Ferry
to Hoshiar pur
Pukelu
Beas River
Nadaun
N.

Fig. 14

The Siwalik formation consists here of tremendously thick sandstones, clays, and conglomerates in frequent alternation. In the Upper Siwalik conglomerates decidedly predominate, in the lower one clays. No well-marked leading horizons have so far been found in this mass.[1]

These Siwalik beds form, in the region I have examined, two enormous anticlines running through the country in a fairly straight line for many miles, even one day's journey in a north-west–south-east direction. They were recognized even by Medlicott as important tectonic lines. He called them after places lying further south—"Gumber fault" and "Budsur fault". The eastern "Gumber fault" will in this paper be called "Jawalamukhi anticline", the western "Chamba Ferry anticline". Chamba Ferry is a point on the Beas River, where the anticline is very clearly exposed. The details in the course and in the shape of the anticlines are represented in Figs. 14 and 15. The geological observations which led to the construction of the anticlines are entered on the map (Fig. 14). I have to draw special attention to some points.

The axis of the Jawalamukhi anticline does not coincide with the steep mountain range, the "Jawalamukhi ridge", at the foot of which the fire-temple is situated, but lies generally at one mile distance from the mountain border.

While vertical bedding appears in the axis in several places, the anticline shows just at Jawalamukhi a fine regularly curved structure.

Towards the north the anticline is curved off a little to the north-west. Transverse faults result from this. The fold is built unsymmetrically with a steep south-west flank and a flat north-east flank. The anticlinal crest must, therefore, under ground remove itself a little to the north-east. To the north of Jawalamukhi the axis of the anticline pitches considerably to the north-west. In the syncline between the two anticlines conglomerates, that is to say, the beds of the Upper Siwalik group, predominate, with very steep dips.

The core of the syncline lies closer to the Chamba Ferry anticline than to the Jawalamukhi anticline. Therefore, supposing similar facies, the Jawalamukhi anticline is curved up more strongly and shows deeper beds than the other one.

The appearance of burning gases (methane gases) is often, but not always, in connexion with oil-rocks. In districts, where oil exists, the appearance of inflammable gases, together with salt-water springs, is considered a reliable indication for oil.

Both gases and salt water are found in Jawalamukhi. Opposite the fire-temple on the southern side of the brook salt-springs come forth below the road. Several further salt-springs are found a little south in the steep slopes east of the village of Jawalamukhi. I also

[1] Medlicott, *Manual of the Geology of India*, 1879, pp. 550–4.

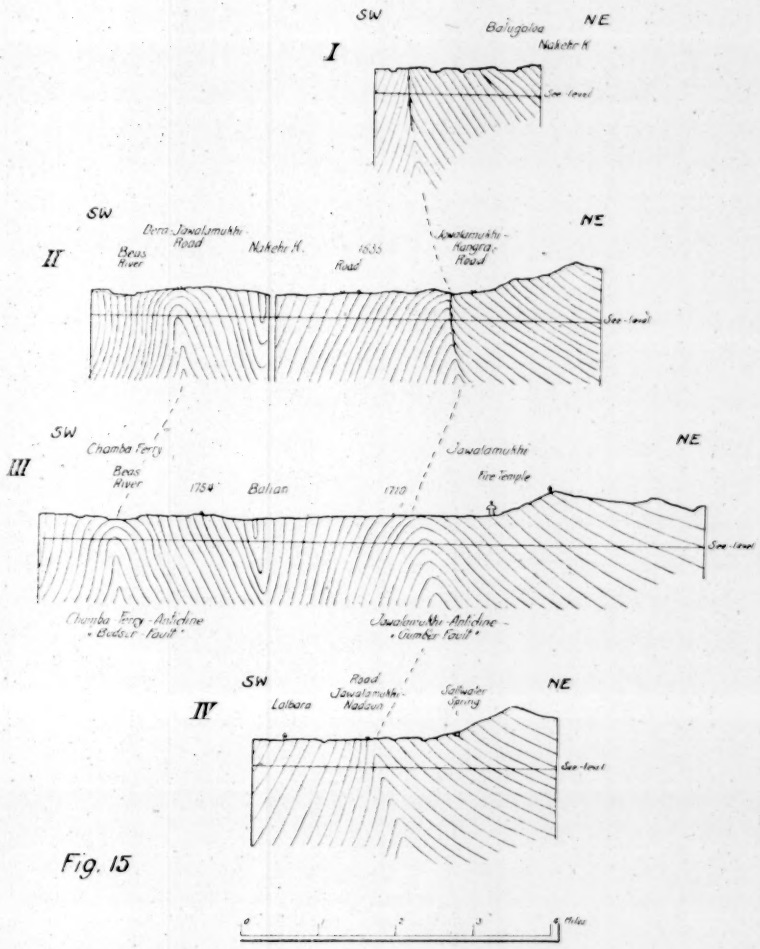

Fig. 15

examined another salt-spring lying wholly isolated from those mentioned just now near Pukelu (or Focolo) about one hour south-east of Jawalamukhi. Two salt-springs have their origin there on the slope of the hills at a height of 80–100 mm. above the plain. They come from practically the same stratigraphical level as the salt-springs and the gases of Jawalamukhi, at exactly the same distance from the anticlinal axis.

There is no doubt of these gases having a geological connexion with the salt-springs. Both probably are in connexion with a salt-mass in depth. Such a salt-mass is, indeed, known. It appears at the base of the Tertiary formation in the mountains about 30 miles

east of Jawalamukhi, where the so-called "black salt" is won at Gama and Drang near Mandi (cf. Medlicott, Mem. Geol. Surv. India, vol. iii, p. 60).

Gas and salt water probably have found their way from depth on transverse fractures, and small transverse faults can be proved here and there (Fig. 14).

"Lissons."

By L. Richardson.

IN the Oolitic rocks of the Cotteswold Hills fissures are of frequent occurrence. Noticeable phenomena are that when snow or hoar frost is on the ground it is usually melted or not deposited along the lines of these fissures and that air is usually continuously expelled from them.

In the neighbourhood of Stroud these fissures through which air is expelled are known as "Lissons". Air is also expelled from other openings in the Oolites—for example, from deep wells. Mr. Charles Upton informs me that from a well 175 feet deep down to the water, at Bownham Cottage on Rodborough Hill, Stroud, the air was always blowing strongly both winter and summer—so strongly, indeed, that a lighted candle was immediately extinguished. People who live on the Cotteswolds are frequently heard to remark that their house is built on "hot rock", meaning thereby that there is a "lisson" or fissure under the house. The effect of this on the house is that it is always damp, and the floors rot and decay in a comparatively short time. There are many houses so situated in Rodborough.

In Mr. Upton's opinion the fissures act as vents for the air which is pressed by barometric pressure into the ground over its whole surface, more readily, obviously, where the rocks are of a more or less porous character, as in the case of the Oolites. The pressure is, of course, continuous, and so also is the outflow, but its effects are only visible in the winter time, when, as remarked above, the snow or hoar frost is either melted or not deposited at all along their line.

The effects of the pressure of a strong wind is often noticeable in wells in the hills. Thus I have been informed that the water in the well that supplies Coates, near Cirencester, usually stands at a higher level when a strong wind is blowing.

REVIEWS.

THE GEOLOGY OF PONDOLAND. By A. L. DU TOIT, D.Sc. Expl. Cape Sheet 28. Geological Survey of the Union of South Africa. pp. 44.

THIS unassuming little memoir of only forty-four pages and the accompanying map contain the record of a piece of work of really remarkable character when we take into account the nature of the country. It comprises a description of the geology of Pondoland and portions of Alfred and Lower Umzimkulu Counties, Natal. This is a region of high plateaus, deeply dissected by river gorges, often with vertical sides; in parts covered by dense forests and only sparsely settled; much of it is native reserve and very little known. Nevertheless, Dr. Du Toit has worked out the geology in much detail, and has discovered many interesting features. The geological formations represented are as follows: oldest of all are the crystalline dolomites of the Marble Delta on the Umzimkulu River, in Natal; then come granites, usually gneissose, and amphibolites of pre-Cape age. The Cape system is represented only by the Table Mountain sandstone, the upper part being absent. The Karroo series is apparently quite complete and very thick. Above this come Lower Cretaceous sandstones and conglomerates, the Embotyi beds, followed by the well-known fossiliferous marine Upper Cretaceous, generally known as the Umtamvuna series. The author says these beds do not occur at the locality from which the name is taken, and he renames them the Umzamba beds. No Tertiary strata have been found. The correlation of the sandstones of Natal with the Table Mountain series, for long in dispute, may now be considered as established. This formation is in many ways remarkably like the Millstone Grit. The Dwyka conglomerate is well developed, and it is considered that in southern Natal its thickness may be as much as 1,500 feet.

The tectonics of the country are very fully discussed, and the existence of some very important dislocations is established, such as the great Umgazana and Egosa faults. However, in spite of the numerous items of interest that might be mentioned, we can only afford space to refer to one special problem, namely, the general nature of the structure determining the coast-line of this area. It is here shown that the view upheld by Suess, among other geologists, that the coast-line is a great fracture, is untenable. The present work confirms Penck's idea that the principal factor was a monoclinal flexure extending along the Lebombo range and then striking S.S.W. through Zululand and along the coast of Natal. Its axis just clears the shore of Northern Pondoland, but is brought inland again by transverse faulting; it must, however, again run out to sea further south. The age of this flexure is not certainly fixed: it is post-Stormberg (i.e. post-Liassic), but its relation to the Cretaceous strata is unknown. Another feature of interest here described is the terrace of marine denudation traceable for a long distance at 1,000 feet above sea-level; other similar shore-lines are traceable up to 2,000 feet, but the latest movement was one of

depression, as shown by the depth to the rock-floor of some of the river mouths. R. H. R.

THE EVOLUTION OF A COAST-LINE, BARROW TO ABERYSTWYTH AND THE ISLE OF MAN, WITH NOTES ON LOST TOWNS, SUBMARINE DISCOVERIES, ETC. By WILLIAM ASHTON. pp. i–xiv and 1–302. London: Edward Stanford. 1920.

THE author has long been engaged in the study of the features presented by the coast-lines considered in this volume, which is based upon his former work *The Battle of Land and Sea*.

There is, especially in the earlier chapters, a good deal of geological matter which is hardly relevant interspersed with information of considerable interest. If the story had been written in a more connected form it would have saved the reader much trouble. Notwithstanding these objections, the book will be found of very high value to anyone concerned with the study of the coastal changes in the area described, for it is a storehouse of facts, many of which are derived from a study of historic documents, including old maps, while others are due to the author's personal observations.

The author maintains that for some thousands of years subsidence has been in progress along the Welsh, Cheshire, and Lancashire coasts to a point provisionally fixed somewhere between Fleetwood and Barrow-in-Furness, north of which elevation is proceeding.

The volume is amply illustrated with diagrams, maps, and photographs, including a large number of ancient maps of the coastal tract. J. E. M.

POPULAR OIL GEOLOGY. By Professor VICTOR ZIEGLER. First edition, pp. viii + 149, 1918. Second edition, pp. xii (not numbered) + 171, 1920. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd.

THE word "popular" prefixed to the title of any book purporting to deal with a particular branch of Natural Science is usually sufficient to deter the average technical man from a perusal of its pages. Possibly its use unconsciously reminds us of all the quaintly worded phrases and ambiguous paragraphs characteristic of volumes written with the intent of making a difficult subject easy, and one is inclined, however erroneously, to prejudge and condemn a work bearing this superscription. It is given to comparatively few scientific men to be able to write a technical treatise which, while intelligible to the general public, at the same time satisfies the most critical expert; the author of the present work, being admittedly one of the "few", is therefore to be congratulated on the production of this volume, which fills a very definite space in the literature of oil-field technology.

The book deals only with the general principles of oil geology, and does not attempt to go into elaborate detail. The early chapters are mainly chemical, and discuss the composition, properties, and origin of oil and gas. The succeeding seven chapters are devoted to

the geological aspects of the science, and chapters 7 and 8 (dealing with the reservoirs of oil and gas, and with the laws of migration and accumulation of petroleum respectively) are especially good. In the discussion of maps and their uses (chapter 9) the author is rather bound down by American convention, and he has not treated this phase of the subject as widely as its universal importance merits; in this connexion brevity may very easily lead to ambiguity or inaccuracy, and the dismissal of "structure maps" and "isochore lines" with two short paragraphs is to be deplored. The chapter on oil structures is somewhat fuller and includes a classification of American oil-fields on the basis of varying tectonic features. The fields are classed with reference to folded structures, monoclinal dips (homoclines), dome structures, faults, and unconformities; such a classification is, of course, open to the criticism that in one field two or more structures may occur, as in the case of Wyoming, where all five of the foregoing features are known, but usually one main type of structure predominates and thus justifies the attempt.

The volume also includes some remarks on the kind of surface indications to be looked for in prospecting for oil; in this connexion it is gratifying to note that the author does not follow the usual lines suggested by some writers who advocate drilling a test well wherever two or more likely seeps of oil and gas can be correlated with supposed anticlinal structure. However favourable the surface indications may be, the geological structure must be proved first of all, and the superficial evidence of oil taken at its true value, i.e. as confirmatory evidence. The sentence in the final paragraph of this section might well have been italicized in order to accentuate its importance, since it is a golden rule for all prospectors: "Drilling should be undertaken only on a favourable geological structure in rocks proved petroliferous." Adherence to this policy not only condemns "wild-catting", but makes for success in oil finding by minimizing risks and eliminating that undesirable factor of "chance".

Although the volume can be read through easily in an hour or so, it contains so much information, concisely arranged and carefully illustrated, that one keeps a copy near to hand for repeated reference where general principles rather than textbook detail are required. We would draw the attention of both author and publisher, however, to the somewhat excessive price charged for a book of this size and description; in its first edition at 11*s.* 6*d.*, and in its more recent edition at 16*s.* 6*d.*, the volume may be said to be anything but "popular". That is the most general criticism made since its publication in 1918. H. B. Milner.

The Geology of the Lomagundi Mica Deposits. By H. B. Maufe. Short Report No. 10, Geological Survey of Southern Rhodesia. pp. 8, with 2 figs. 1920.

THE country rock of the district consists of mica-schists and quartzites, believed to be an unusually highly metamorphosed facies of the Lomagundi system, which is considered to be

generally equivalent to the Transvaal system. The schists are invaded by a granite comparable with the Newer Granites of the Cape Province. The mica occurs in a remarkably regular manner in large " books ", usually on the walls of pegmatite dykes, which consist mainly of felspar, with a little quartz, and often with tourmaline and rarely large prisms of beryl. The mica is wholly muscovite, usually either the ruby or bottle-green variety, sometimes clouded and silvery, the books varying from four to five inches in diameter to as many feet, and generally not more than six inches thick. The visible supplies are large and the industry shows much promise.

REPORTS AND PROCEEDINGS.

Edinburgh Geological Society.

January 19, 1921.—Mr. D. Balsillie, B.Sc., F.R.S.E., Vice-President, in the Chair.

1. " An Address on ' The Topography of Spitsbergen '." By John Mathieson, F.R.S.G.S.

Mr. Mathieson stated that Spitsbergen was discovered by the Dutch in 1596, while they were attempting to find a North-west Passage. The archipelago consisted of a group of five large islands, with numberless small ones, and the estimated area was 25,000 square miles, or half the size of England. Although within 700 miles of the North Pole and 1,620 miles north of Edinburgh, the summer climate was not much colder than the Shetlands, and the winter was much milder than either Moscow or Winnipeg, a condition which was entirely due to the influence of the North Atlantic drift. The largest island on the west, known as the mainland, was deeply indented with many fiords, the largest, Icefiord, extending over 50 miles. Flanked against the mainland by Prince Charles Foreland —first explored by Dr. W. S. Bruce—which had an area of 250 square miles, with a mountain range rising to nearly 4,000 feet. The general appearance of the country was indicated by the name, which meant " sharp-peaked, rocky mountains ", rising generally to a height of 2,500 to 3,500 feet, with one peak in the south over 5,000 feet, and Mount Newton in the north, 5,676 feet. The west was comparatively free from ice, but the north-east of the mainland was covered with a sheet 1,000 feet thick, while the North-east Land was covered with an ice-sheet 1,700 feet thick. Barents and Edge Islands were practically free from ice. Stor Fiord was selected by the Russo-Swedish authorities to measure an arc of meridian (1898–1902) which extended from South Cape to the Seven Islands, a distance of over 300 miles. Glaciers were receding on the west, but there were still many flowing to the sea, and calving icebergs during the summer. The rate at which these glaciers moved had not been fully examined, but one measured this year showed about

100 feet per annum. A specially interesting feature of the landscape was the raised beaches, which could be traced to over 300 feet above sea-level. For 200 years after its discovery Spitsbergen was the whale-hunting place of the world, one company alone capturing 32,000 whales in 46 years, and until recent times it was the trapper's hunting ground for reindeer, fox, bear, seal, and walrus. But now that the fauna had been ruthlessly exterminated, there was a rush to exploit the rich coalfields, which occurred in the Tertiary, Jurassic, and Carboniferous periods, and were estimated to contain over seven billion tons. The flora consisted of 140 or so of Arctic flowering plants, lichens, and mosses, affording ample food for reindeer, and during the summer a grazing for cows at mining centres.

2. "An Exhibition of Sketches Illustrating the Scenery of Spitsbergen." By H. M. Cadell, J.P., D.L., B.Sc., F.R.S.E., of Grange, Linlithgow.

Mr. H. M. Cadell exhibited a series of water-colour sketches and panoramas he had drawn during a visit to Spitsbergen last summer, showing the character of the scenery of the Ice Fiord district, in central Spitsbergen, and of the shores of Bell Sound and Lowe Sound, further to the south. The prevailing colour of the mountains was of a dun or brownish tint, and the lower parts of the cliff faces were buried under long scree slopes of crumbling debris. The geological structure was for the most part quite simple, and there were few faults or folded strata over wide areas. The most important line of fault or upthrust skirted the west coast, and produced a more picturesque kind of scenery than that of the undisturbed regions. The Middle Hook, a jagged mountain at the mouth of Lowe Sound, displayed the upturned strata in a magnificent cliff face at Axel's Island, which, unlike most other spots, was partly covered with green vegetation that added to the artistic effect of the landscape.

ROYAL SOCIETY OF EDINBURGH.

February 7, 1921.

"Depths and Deposits of the Weddell Sea." By J. M. Wordie, M.A., F.G.S.

A description of some of the oceanographical work of the Shackleton Antarctic Expedition, 1914–17. "Blue mud" is no longer considered a suitable term for the deep-sea deposits off Antarctica; "glacial mud and clay" more fitly expresses the poorness in organic remains and the lack of any sorting out of the constituents according to size characteristic of other glacial deposits. Rock fragments were frequently got whilst sounding or dredging; and from a study of the prevailing drift in the Weddell Sea it was clear that they must have had their source in Coats Land. They offer the only evidence of its geological constitution, and suggest affinities with Victoria Land rather than with Graham Land.

The soundings made almost every day led to the discovery of an unexpected shallow area in the south-west. The continental shelf here has a breadth of at least 100 miles, and was followed over a distance of 300 miles. The depths clustered either about 180–190 fathoms or 250–260 fathoms; the back and forward drift of the *Endurance* showed that the structure of the sea bottom there was a series of stepped plateaus. The abnormal depth of the continental shelf round Antarctica is probably due to earth movements. The position of the isobaths for 1,000, 1,500, and 2,000 fathoms, and the drift of the ice practically settled the vexed question of "Morrell's land", whose existence must now be considered highly improbable.

Mineralogical Society.

January 18.—Mr. A. Hutchinson, Vice-President, in the Chair.

A. F. Hallimond: The Olivine Group.

Since the discussion of the densities by Thaddeef in 1896, and of the optical constants by Backlund in 1909, numerous additions have been made to the published data. These have been collected, and the most probable values for the pure compounds obtained. The molecular volume of monticellite is slightly greater than the mean between forsterite and γ-Ca_2SiO_4. For the complex mixtures the density and mean refractive index yield additive relationships, but the birefringence and axial ratios follow no additive law. The conditions of plotting which must be observed if the variation of an additive property with composition is to be expressed by a straight line were briefly summarized.

W. A. Richardson: A Method of Rock-analysis Diagrams based on Statistics.

Oxide variation diagrams, similar to those employed by Dr. Harker, can be used for expressing the chemical relations of rock groups and individuals. The diagrams obtained from plotting Iddings' selected analyses gave the maximum variation for all rocks.

L. J. Spencer: Identity of Trechmann's "β-tin" with stannous sulphide.

A re-examination of the original material described by C. O. Trechmann in 1879 as an orthorhombic modification of tin proved that he made his crystallographic determinations on crystals of one kind (viz. stannous sulphide), whilst the chemical analysis was made on crystals of another kind (viz. metallic tin). Tin, is therefore dimorphous and not trimorphous, "white tin" being tetragonal and "grey tin" cubic. Orthorhombic crystals of stannous sulphide (SnS) and tetragonal crystals of iron stannide ($FeSn_3$) from tin furnaces, and rhombohedral crystals of tin arsenide (Sn_3As_2) isolated from a tin-arsenic alloy were also described.

CORRESPONDENCE.

THE GLACIATION OF IRELAND.

SIR,—Professor Kendall's criticisms of my paper on Irish Eskers include various specific objections.

(1) He complains of lack of deliberation. The first of my many visits to Irish eskers was in 1887. At my time of life I cannot hope for equal further deliberation.

(2) He objects that the paper contains very few new field observations. I should have been less surprised by the complaint that the paper quoted too lengthily from my field note-books; seventeen pages out of thirty-five are occupied by detailed descriptions of representative eskers and mainly of their internal structures; and I justified this length on the ground that the previous literature "deals mainly with their distribution and general structure" (p. 116), and but little with their intimate structure.

(3) Apparently I should have quoted my notes at still greater length, for compression has led to misunderstanding. In accordance with Professor Kendall's habit of regarding as simple mistakes views he does not accept, he so dismisses the attribution of the crescentic series of eskers around the northern end of the Slieve Bloom Mountains to ice from those hills. He says I "might have been spared this mistake" had I consulted G.S.I. Memoir, No. 117–8. I had not only consulted it but quoted it, for its account of some "anastomosing eskers". Professor Kendall's quotation from that Memoir supports my conclusion; for the rarity of Galway granite on the northern in contrast to its abundance on the southern slopes of the Slieve Bloom Mountains, is most easily explained by its entrance having been hindered or by it having been subsequently swept away by local ice. The existence of this local drift is recognized in the Memoir, No. 127, p. 26.

As regards the position of Roscrea, my sentence: "The Roscrea, Clonaslee, Mountmellick, and Maryboro Eskers were probably formed by ice which flowed down the northern slopes of the Slieve Bloom Mountains"—was an attempt to indicate in two lines the relations of over 30 miles of esker. They are part of one crescentic series around the northern end of the range. Moreover, the term Slieve Bloom Mountains is sometimes used (e.g. Phillips, *Atlas of Comparative Geography*, and the map used in Carvill Lewis' *Glac. Geol. Gt. B. and I.*, 1894, opp. p. 83), to include the geological continuation of the range south-west of the Roscrea Gap, and in that sense the south-western or Roscrea end of the series is north of the range.

I referred so briefly to this series because unusual detail had been given of its internal structure (e.g. Memoir, No. 126, p. 22).

(4) The origin of the boulders is but exceptionally referred to in my paper because it is so seldom that the last direction of movement can be inferred from the nature of the included rocks. Thus, the granite in the kame near Barony Bridge, Tyrone, gives

no clue to the movement of the ice which made the kame. No doubt the area was invaded by ice from the north-west; but the movement at the time of the kame formation is not shown by the original home of the boulders. According to Professor Cole (*Reg. Geol.*, iii, p. 329, received after my MS. had been sent to the Royal Society) the materials of the Tyrone eskers were distributed from south to north. I am sorry to differ from Dr. Charlesworth regarding an Ulster kame, and if he will publish his evidence I will carefully consider it; but I find it difficult to realize how this kame, steeply descending the hill to the north and with its curves concave to the south-east, can be due to ice from the north-west, although much of its material originally came from that direction.

Professor Kendall claims that by insertion on Fig. 9 of two arrows from Professor Sollas's map I have unintentionally contradicted the statements in the text that the ice flowed in the opposite direction. I described the Dunmore eskers in reference to the claim that they were deposited within ice which was moving at right angles to their trend. For, if so, and if glaciers have any power of erosion, it would appear clear that the eskers must have been formed after the ice had ceased to flow across their sites. I agree with Professor Sollas that the course of the ice was along a line trending north-west and south-east; and to show that Professor Sollas adopted that course I inserted the two arrows from his map. The legend of the figure quotes them as from Professor Sollas, and as marking "the course of the ice movement", not its direction along that course. It is made clear in the text that I consider that the direction adopted should be reversed; but I left the arrows to prevent any possible suspicion that I was claiming Professor Sollas's agreement with the direction of movement as well as with its course. Whether the ice moved from or towards the north-west being immaterial to the formation of these kames, I mentioned my conclusion and the nature of the evidence, but did not give it in detail.

(6) Professor Kendall also objects to my insertion on a sketch map (Fig. 11), showing the relations of the eskers to the 300 foot contour of some arrows indicating the ice movement according to Mr. W. B. Wright. The first objection raised is that Mr. Wright's map refers to the maximum extension of the ice and not to the stage to which I assign the eskers. But the theory which I was proposing to amend is that the eskers were formed by rivers within the great ice-sheet; and one fact I hoped to show by the figure was that many of the chief eskers trend across the line of the movement of the ice, and were formed on its margin during its retreat, and not within it. To that argument the map is relevant. The second objection is that I have unduly magnified the lines. In order to prevent attaching to Mr. Wright's lines a significance as to details greater than the scale of his map would warrant, I ended the lines to the south against the hill country of Slieve Bloom, between

Lough Derg and South Kildare. I inserted only thirteen out of the thirty-eight lines marked by Mr. Wright, as they were sufficient to show the general view as to the ice-movement in Mid-West Ireland. Eleven out of the thirteen lines follow the originals precisely ; the two easternmost are rather generalized to show the movement east of Lough Ree and trend rather farther to the west than the nearest corresponding of Mr. Wright's lines ; but this difference does not affect the argument, as their direction in the original is athwart the chief eskers, and is inconsistent with the formation of the eskers by intra-glacial rivers.

Origin of Boulder Clay.—Passing to the general question of the origin of boulder clay, Professor Kendall states that I intend "a general assertion of the marine origin of all boulder clay". No such assertion is intended. I briefly stated my view of its origin in *Geology of To-day* (p. 227). So far from claiming all boulder clay as marine, I described its formation in an ice-blocked depression at the head of the Fulmar Valley in Spitsbergen. It is, however, sometimes subaqueous, deposited either in lakes or in quiet arms of the sea as off the Sefstrom Glacier. Each case must be determined by the local evidence, and where the boulder clay contains contemporary foraminifera, the possibility of its origin as marine mud must be considered. The foraminifera cannot be simply dismissed as derived from older rocks when they have been determined by Mr. Joseph Wright. I once sent him some Essex boulder clay, and he reported a number of species as derived from the Chalk and a list of others as indigenous to the clay. Mr. J. Wright is not likely to make the mistake suggested.

That the belief in the marine origin of boulder clay has been and is rejected by the majority of British glacial geologists is fully admitted in my paper. Professor Kendall asks to whom I referred as upholders of the marine origin of boulder clay. Professor Bonney's Presidential Address to the British Association in 1910 shows that the marine theory has been consistently supported by high authorities. My own partial acceptance of the view has been by no means consistent, for I at first regarded all boulder clay as terrestrial, and was only gradually led to the view that some of it is marine. Amongst men with an intimate knowledge of the boulder clay of the south-west of Scotland, and who regard it as a marine deposit, may be mentioned Mr. J. Neilson, for the Glasgow district, and Mr. John Smith, after his detailed study of the Ayrshire Drifts. The increasing faith in isostatic oscillations has also encouraged the probability of a glacial subsidence. In recent years there seems to have been a decided trend toward the opinions that the boulder clay has not yet been satisfactorily explained, and that some of it is marine. The consequences of that conclusion are not so startling as Professor Kendall suggests, for until the shell beds at 1,300 feet on Three Rock Mountain, near Dublin ; at Moel Tryfaen, Oswestry,

and Clava are adequately explained without submergence, there is nothing improbable in a 400 foot submergence of the Central Plain of Ireland. Carvill Lewis adopted a 400 foot Irish submergence during the advance of the ice (*Glac. Geol.*, p. 148).

The chief difficulty in the marine origin of Irish boulder clay is its poverty in marine fossils; but the references quoted in my paper show that marine fossils are widely scattered in the Irish drifts. They are rare, and to explain their rarity I quoted from men so experienced in polar biology as Dr. Nansen and Mr. J. Murray to show that under some conditions life is absent from the Polar seas. Dr. Nansen's statements that the floor of parts of the Arctic Sea are lifeless are not refuted by Gran having found the opposite in "samples taken later during the expedition". Similarly, in the Antarctic, Murray's statement that the shore deposits at Cape Royds contain no vestige of life is not refuted by the occurrence of shells elsewhere and in beds which, owing to the scarcity of life along the shore, Hedley and Priestly reject as beaches and attribute to upheaval and upthrust. Even in the Swedish drifts, though shells are usually abundant, the clays are sometimes sterile over large areas.

The marine origin of the Irish boulder clay is a subsidiary issue; the object of my paper was to show by a description of the internal structure and field relations of representative Irish eskers, that the most important were not formed along intra-glacial rivers, but on the margin of the ice, where it ended in a sheet of water. Most of the eskers in fact are kames, not osar. I fully recognize that the evidence for the sheet of water being the sea is less clear than that as to the nature of the eskers. I only advance the view that it was the sea as being more probable than that it was a series of glacial lakes; and there is nothing in Professor Kendall's note to modify that opinion. I regret his adoption of a tone of discussion which seems to me as out of date as the view that all boulder clay may be simply explained as *moraine profonde*.

J. W. Gregory.

THE AGE OF THE SHENLEY LIMESTONE.

Sir,—It is fortunate that the Shenley echinoderms have received expert examination, and our thanks are due to Professor H. L. Hawkins for his note on the subject in your February issue (p. 57). I will, however, ask for temporary suspension of judgment in respect to his deductions as to the age of the deposit.

As the result of recent work, I shall be able to communicate to the Geological Society during the present session a paper containing much new evidence, both stratigraphical and palæontological, to prove that the limestone is in its proper position below the Gault, and that the Gault of the section belongs to the Lower and not, as supposed, to the Upper Gault.

G. W. Lamplugh.

St. Albans.
February 10, 1921.

OBITUARY.

Sir Lazarus Fletcher, Kt., M.A., F.R.S., etc.

BORN MARCH 3, 1854. DIED JANUARY 6, 1921.

BY the death of Sir Lazarus Fletcher, of heart-failure, at Grange-over-Sands, Lancashire, on January 6, mineralogical science has lost one of its ablest exponents. The famous series of Introductions to the Study of Meteorites, Minerals, and Rocks respectively, which he prepared at intervals after he became Keeper of the Mineral Department in the British Museum, have been the most efficient means for bringing the elementary principles of the science before the public, while for advanced students his treatise on double refraction, *The Optical Indicatrix and the Transmission of Light in Crystals*, has revolutionized the teaching of physical optics. Although he has thus been, and will continue to be as long as these guide-books are published, the means of arousing an interest in minerals in others, it may be said of him, as of many other mineralogists, that he was a mathematician first and a mineralogist afterwards. In order to appreciate fully the beauty, variety of form, and mutual relations of minerals, some knowledge of mathematics, physics, and chemistry is essential. Fletcher's distinguished career at school and college gave him to perfection this preliminary training, and it was not until after he had taken his degree at Oxford that he was first led to the study of crystals and thence was attracted to minerals.

Sir Lazarus Fletcher was born at Salford, Manchester, on March 3, 1854. At the age of eleven he entered the Manchester Grammar School, and during the next four years displayed such brilliance as a scholar that, instead of being launched upon a business career in that commercial centre as contemplated by his father, he was allowed, by the advice of the headmaster, to continue his studies, with the result that at the early age of seventeen he obtained a Brackenbury Science Scholarship at Balliol. His career at Oxford was signalized by first classes in mathematical Moderations and in the final schools of mathematics and natural science. After taking his degree he became demonstrator in physics to Professor Clifton in the Clarendon Laboratory, and Millard lecturer in physics at Trinity College, and was made a Fellow of University College. His work in the Clarendon Laboratory attracted the notice of Story Maskelyne, who at that time was acting both as Professor of Mineralogy at Oxford and as Keeper of the Mineral Department of the British Museum in London; and in 1880 Fletcher, after serving two years as assistant, on Maskelyne's retirement became the Keeper of the Department. It was at first a troublous heritage to which he had succeeded, for almost immediately he was confronted with the formidable task of removing the mineral collections from

Bloomsbury and arranging them in their new home at South Kensington. This transference and rearrangement skilfully accomplished, time was afforded him for the preparation of the celebrated guide-books and the selection and arrangement of the series of specimens to illustrate them. In the intervals of this work he was able to prosecute his crystallographic investigations of minerals and those chemical analyses of meteorites which are characterized by the meticulous care which he brought to bear upon any scientific work he undertook. In recognition of his services to science, in 1889 he was elected a Fellow of the Royal Society, in 1894 was chosen as President of the Geological Section of the British Association at its meeting in Oxford, and in 1912 was awarded the Wollaston medal of the Geological Society. After the retirement of Sir Ray Lankester in 1909 he became the Director of the Natural History Museum, and in 1916 was knighted. A few years before his promotion to the Directorship of the Museum he had been attacked by that heart-trouble which had the effect of sapping his energies during the rest of his career, and was eventually the cause of his death. This took place in his native country, the North of England, to which he had returned with his wife and daughter after his retirement from the Museum less than two years ago.

Fletcher's genial and amiable disposition and dry North Country humour endeared him to friends and colleagues. As characteristic examples of this quiet humour the following passages from the address delivered to the Mineralogical Society on his retirement from the Presidency in 1888 may be quoted: "It may be cast in our teeth that the volume (of the Society's Journal) is small, but one can proudly and truly retort that few volumes of the same size furnish so vast an amount of heavy reading," and again: "The Ancients, not unwisely, refrained from the invention of printing; they recognized . . . that their duty to posterity was to transmit to it only their masterpieces; when even these became burdensome, an incendiary, doubtless a librarian, quickly reduced their volume." Fletcher was put in a position of authority over others early in life, and happy was the lot of those who were fated to work under his direction. No bitterness or ill-feeling ever arose to mar their friendly intercourse, for irritability of temper was not in his nature, and the scrupulous regard for fair-play which characterized his relations with his assistants may be judged from the following extract from one of his reports: "After the Assistant has come on duty I have encouraged him in precise rather than in voluminous scientific work, and have taken care that he has had the sole credit of any work he has done. Further, to avoid the possibility of development of difficulties between the Assistant and myself, no one has been allowed to give me the least help in any scientific work which I have myself taken in hand." For the prosecution of such work he possessed in the highest degree that capacity for taking infinite pains which is supposed to be the attribute of genius. The elaborate

method which he adopted for the analysis of meteoric stones, and which he described in detail for the benefit of others engaged in similar work, was one which it is to be feared could appeal to few who were not possessed of his own skill and infinite patience. For many years previous to his retirement from the Museum he had devoted much thought to the problem of the classification of minerals, and with his usual thoroughness had critically examined every system which had been entertained during the last 200 years. During the last year of his life he had made some effort towards the completion of his task, and it is to be hoped that it may be possible to prepare for publication something of interest, at any rate from a historical point of view, from the manuscript which he has left behind.

The Mineralogical Society owes Fletcher a special debt of gratitude for the work which he did on its behalf. As one of its earliest members, its President from 1885 to 1888, and its Secretary for more than twenty years afterwards, he worked indefatigably first to establish the Society on a firm basis, and then to promote its interests.

Loyalty to friends and to any institutions with which he was or had been connected was one of the guiding principles of Fletcher's life. After he had attained to an assured position he was ever ready to help those from whom he may have received benefit in his youth, and the school and college in which he was educated. No one in trouble ever appears to have appealed to him in vain. Partly as a consequence of this open-handed generosity and partly, no doubt, from choice, his home life was always a very quiet and simple one. He was twice married, first to Miss Agnes Ward Holme, who died in 1915, leaving one daughter, and afterwards to her sister Edith. His wife and daughter survive him.

G. T. Prior.

James Reeve, F.G.S.

Born May 12, 1833. Died December 19, 1920.

Mr. James Reeve, F.G.S., Consulting Curator of the Norwich Castle Museum, died at Norwich on December 19, 1920, in which city he was born on May 12, 1833.

The 27th Annual Report of the Committee of the Norfolk and Norwich Museum for 1851 records that James Reeve, Assistant, had been appointed Curator in succession to Mr. H. J. Bellars, who had resigned. This appointment he held with conspicuous success until 1894, when the collections were transferred to the Town Council of Norwich and housed in the splendid buildings known as the Norwich Castle Museum, which was opened to the public on October 23, 1894, by the Duke and Duchess of York (the present King and Queen). Mr. James Reeve received the appointment as Curator under the Town Council, and by his energy and unrivalled knowledge